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Woodwardian Museum Notes.

I.—New Trilobites from the Bala Beds of Co. Waterford.

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(PLATE III.)

AMONGST the numerous species of Trilobites which occur in the impure limestones of Bala age in Co. Waterford, two new and undescribed forms were recently found by me in the course of a preliminary examination of that area. At present the exact horizon of the beds has not been determined, but they may undoubtedly be referred to the Bala series. One of the new species belongs to the genus *Cybele*, and is a most bizarre form, with a long, snout-like projection of the anterior margin. The other is a new species of *Trinucleus*, bearing some resemblance to Angelin's *T. bucculentus*, which occurs in beds of "regio Ba" in Norway.

The following descriptions give in detail the characters of the new species.

Cybele tramorensis, n. sp. (Pl. III. Fig. 1.)

The only specimen of this new species was discovered in the townland of Quillia, and consists of a head-shield which is unfortunately not quite perfect; but it shows sufficient new characters to warrant us in considering it specifically distinct from any hitherto described.

It is semicircular in outline, with an anterior projecting process, and it is about twice as wide as long. The glabella is oblong, and the sides converge a little towards the anterior end, which is in the shape of a slightly convex-forwards arc. The glabella is gently elevated above the cheeks, but is rather flattened in front. The side-furrows are four in number, including the neck-furrow. The first or anterior one is merely an elongated deep pit, which does not join the axal furrow; it is directed forwards at an angle of 45° to the axis of the glabella. The frontal lobe is thus only marked off by this pair of isolated pits. The second or middle side-furrow is also an obliquely-set indentation inclined to the axis of the glabella at an angle of about 70° and directed forwards and outwards; at its inner end it is deep, but suddenly decreases in depth when traced outwards, and leads into the axal furrow by only a very shallow groove. The first lobe which it limits posteriorly is a short rounded ridge, not marked off very distinctly by the axal furrow at its outer extremity.

The third furrow is parallel to and as deep as the second at its inner end, but does not decrease in depth to the same extent in its outer portion. The second lobe has a rounded termination, but, except for this character and a greater length, is much like the first. The neck furrow is parallel to the third furrow, and rather deep along its whole length; it bounds posteriorly the third lobe, which is more prominent and better defined than the second lobe. The lateral extension of the neck furrow and the neck lobe are not well preserved, but the latter appears to have been slightly wider than the third lobe. The axal furrows are shallow. In front of the first lobe they are practically non-existent, for the ocular ridge and "terminal pit" lie across their forward course.

The surface of the glabella is sparsely strewn with a few large tubercles which do not show any symmetrical arrangement. Smaller tubercles are visible between the large ones, and minute pits also occur, but not very numerously. Each of the two hindmost lobes bears two tubercles of large size; and the anterior end of the glabella shows an aggregation of large and small tubercles without any linear or other regular order. The anterior end of the glabella is steep and abruptly truncated; it is separated from the peculiar anterior margin by a very faint depression. The middle portion of the anterior margin projects to a distance of just half the length of the glabella as a central flat spatulate process, in the specimen before us bent upwards at an angle of 45°. It is traversed by five low, but distinct, flattened ridges, separated by narrow and faint grooves which deepen in their forward portion. These ridges and grooves radiate from the front of the glabella in a fan-shaped manner; and on the anterior border of the spatulate process the ridges end as five short blunt projections. The border of this process has, therefore, a notched appearance. Two or three much fainter ridges and grooves occur on each side of the five central ones, producing a slight serration of the margin.

A clue to the origin and formation of this peculiar process seems to be given by the presence in a few species of *Cybele* of several radiating spines of greater or less length on the anterior margin; and if, for instance, in *C. coronata* (Schmidt) these spines were fused laterally together so as to leave only their tips free we should get a snout-like projecting process very similar to that in the Irish form. That such is the origin and composition of the spatulate process is rendered additionally probable by the presence of the radiating ridges, for they would represent the last traces of the formerly separate spines. In *C. coronata*, however, the spines diverge more strongly than do the ridges on the spatulate process of this new species.

Just outside the point on the anterior margin from which this process starts forwards, and slightly in front of the anterior outer angles of the frontal lobe of the glabella, there rises a low but well-marked rounded ridge which runs forwards and a little outwards, to become a prominent rounded tubercle projecting beyond the margin, which has here a width equal to about half the length

of the spatulate process. This marginal tubercle corresponds with the "side-tubercle" of Schmidt, which is present in most species of *Cybele*, but is wanting strangely in *C. coronata*, to which the Irish form is most closely allied.

Close beneath the posterior outer slope of the ridge leading up to the "side-tubercle" is a deep circular pit, called by Schmidt the "terminal pit." It lies at the anterior end of the axal furrow, and is a most conspicuous object on the head-shield. According to Schmidt it corresponds with a similarly-situated pit in some species of *Cheirurus*, and is concerned in the attachment of the hypostome. The pit is, in the form under description, bordered posteriorly by a strong horizontal ridge—the ocular ridge—which runs outwards to the eye, at right angles to the axis of the glabella, and opposite to the first side-furrow. It is probably parallel to the posterior margin of the head-shield, but of this one cannot be quite certain owing to the imperfect preservation of that part. This ocular ridge has a row of minute pits close along the base of each side, and it also bears two large tubercles.

The anterior edge of the cheek runs outwards and backwards in a nearly straight line to meet the outer extremity of the ocular ridge at the small eye which is situated at a distance from the side of the glabella nearly equal to the width of the frontal lobe. The triangular space thus enclosed between the "terminal pit," the ocular ridge, and the front border of the cheek is inclined slightly downwards and forwards. It bears a central tubercle, and is also granulated and minutely pitted. Behind the ocular ridge lies the main portion of the cheek; its inner corners are almost rectangular, but its outer side has an outward as well as a backward trend from the eye, so that the width of the cheek measured along its posterior margin is more than half as much again as that of the glabella. At the bluntly pointed outer posterior angle the cheek is rather sharply bent down, and this downward bend seems to extend forwards some way along the outer side; otherwise the cheek is almost flat and horizontally extended. On its surface it carries two or three large tubercles and many smaller ones, in addition to numerous minute pits.

In the absence of any portions of the thorax or pygidium, a fuller description of the species at present is impossible.

On reviewing the previously described species of *Cybele*, one sees that this Irish form more closely approximates to *C. coronata* (Schmidt) than to any other. With this species, which occurs in C_2 (Brandschiefer) of Estland, it agrees in the axal furrows being very shallow, in the position and characters of the side furrows, in the strong horizontal eye-ridge with side punctures at the level of the first side-furrow, in the position of the eyes, in the general shape of the cheeks, and in the relative dimensions of the various parts. If my interpretation of the origin of the spatulate process is correct, the long "crown" of spines in *C. coronata* may also be regarded as a point of similarity. The Irish form, however, differs from this species in the glabella not being strongly arched forward in front,

in the smaller size of the frontal lobe, in the presence of the "side-tubercles," in the greater width of the anterior border, in the triangular portion of the cheek that lies in front of the ocular ridge not being vertical, in the greater depth of the terminal pits, in the smaller width of the cheeks, in the irregular disposition of the tubercles on the glabella and cheeks, in the presence of two tubercles on the eye-ridge, and especially in the spatulate anterior process. These differences appear to me to be quite sufficient to justify a new species being formed for the reception of the Irish *Cybele*; and from the locality near which it was found it may appropriately be designated *Cybele tramorensis*.

DIMENSIONS.

Length of glabella from neck furrow to anterior extremity	·350 inch
Breadth of glabella at level of the second lobes	·275 inch
Greatest width of cheek (measured along posterior border)	·475 inch
Distance between "terminal pits"	·350 inch
Length of anterior "spatulate process" (measured from anterior extremity of glabella)	·175 inch
Distance of eye from side of glabella (measured along ocular ridge)	·250 inch

Trinucleus hibernicus, n. sp. (Pl. III. Figs. 2–7.)

This new species of *Trinucleus* is extremely abundant in that portion of the Bala series which is exposed at Newtown Cove, near Tramore. The average length of this form is a little more than half an inch; a rolled-up specimen measured ·6 inch, but some of the head-shields found were larger than that of this individual, while a few were smaller; but these differences may only be due to age. The head-shields, which are by far the commonest part of the trilobite preserved, very rarely have any of the body-rings attached. The head-shield is rather longer than the thorax and pygidium together, according to the above-mentioned rolled-up specimen, but in those individuals in which the head-shield has a semicircular outline it is rather shorter.

There appear to be two forms of this species which are characterized by the shape and certain features of the head-shield. In one form this part has a slightly pointed front and approaches a parabolic shape. In the other form it is broader and semicircular. Certain other differences in the length of the glabella, the sulci in the fringe, and lateral spines will be severally pointed out in the description of these parts. Probably these two forms represent the sexes, the pointed one being the male (?).

The head-shield in the male is less than twice as wide as long, but in the female the width is to the length as 9 : 4. The glabella consists of a prominent globular frontal lobe set on a narrow basal stalk. This large globular frontal lobe invades more than half the width of the fringe, and in the male slightly overhangs also the other half. It bears a central tubercle. Behind this lobe the glabella is sharply contracted to form a narrow parallel-sided stalk, about half the width and less than half the height of the frontal lobe, but equal to it in length. This stalk-like portion has two pairs of small transversely elongated lobes. The anterior pair is situated

close under the great frontal lobe, which has its posterior border notched by the first pair of furrows. The posterior pair of lobes, which are slightly smaller and less distinct than the anterior, are situated about half-way between the neck lobe and the anterior pair.

The neck lobe is very narrow, and is only marked off from the rest of the glabella by a shallow, transverse groove. Behind the cheeks the neck segment is distinguishable as a gradual elevation of the posterior margin of the neck furrow, which has a steep anterior slope. But as the neck segment is followed towards the outer angle of the cheeks it assumes greater prominence, broadens and becomes a rounded distinct ridge usurping two-thirds of the width of the neck furrow.

The triangular cheeks are much inflated and rise abruptly from this furrow to rather more than half the height of the frontal lobe, from which they are separated by a deep axal furrow. The axal furrow is in this part narrower than it is posteriorly, where it widens out and becomes less deep, so as almost to attain the width of the glabella stalk. This widening of the furrow is due to the sudden contraction of the glabella behind the frontal lobe, and to the inner side of the cheek making nearly a right angle with the posterior edge of the head-shield instead of following the outline of the glabella. The cheeks do not overhang the fringe, but rise from its inner border with a pronounced convexity; the inner and posterior borders of the cheeks form steep and abrupt slopes.

A tubercle similar to that on the frontal lobe is situated on the most elevated point of each cheek. A fine reticulated kind of sculpture, with elongated meshes, ornaments the cheeks and glabella. There is no facial suture or "eye-line" visible.

The fringe surrounding the front and sides of the head-shield is parabolic in outline in the male, but semicircular in the female, as already mentioned. It is of constant width throughout, except in front, where the glabella invades it to half its width, and at the posterior angles, where it slightly expands. Its average width is a little less than one-third the length of the glabella. The upper surface of the fringe, which is gently concave, with a rounded edge, bears 20–26 radially-elongated deep sulci, of which the anterior are rather broader and more open than the posterior. Along the bottom of the 14–18 median sulci lie three conical pits (Pl. III. Fig. 6): the anterior one is smaller than the other two and does not communicate with the lower surface of the fringe. The two other larger pits, of which the posterior is of greater size, communicate with the lower surface by means of a minute pore at their base—the apex of the cone—which leads into a similarly arranged and corresponding pair of inverted conical pits opening on the under surface of the fringe. Whereas the anterior pit is only present in the median 14–18 sulci, the two larger pits with their corresponding pair of lower-surface pits are present in all the sulci of the fringe. In the last four or five sulci on each side the two pits on the upper surface approach more closely to each other until in the last two or three sulci they have actually fused more or less into one. In the

cast of the lower surface of the fringe (Fig. 5) it is seen that there are only two pits present in each sulcus, and that these do not fuse together in the few last radial sulci on each side as the corresponding ones do on the upper surface, whereas in the anterior radial sulci on the lower surface the two pits approximate or even tend to fuse into one. There is also seen on the lower surface a marked smooth space, nearly equal in width to the sulci, running behind them in a pointed arch and separating them from a single series of small pits which do not show on the upper surface. The median front portion of this series is situated on a triangular depressed area which posteriorly is extended into narrow vertical prolongations, separated from the rest of the fringe by a continuation of the unpitted space. These prolongations, which are in fact the upwardly-bent inner margins of the fringe, decrease in width till they end at the base of the genal spines and bear the single row of small pits. What appears, therefore, in the impression as a cushion, corresponds to an actual concavity on the lower surface of the fringe. The genal or lateral spines start from the outer posterior angles of the head-shield; each spine is stout at its base, but tapers rather rapidly to a point; it is also grooved along its whole length and has an outward curve. As far as I can draw conclusions from the fragmentary material at present at my disposal the spines in the male are longer than those in the female. The degree of outward curvature is also liable to variation.

The thorax with its six body-rings has the axis strongly convex. The pleuræ are horizontal as far as the fulcrum, but are at that point bent gently downwards and slightly backwards. The fulcrum is situated at a distance from the axis of rather less than two-thirds the whole length of the pleura. A shallow furrow starts from the anterior inner angle, and runs to the outer posterior angle of each pleura, increasing in breadth as it is traced outwards. It is bordered in front by a narrow rounded margin of constant width which is formed by the raised anterior edge of the pleura. The posterior part of the pleura is abruptly elevated behind the oblique furrow, but slopes down gently to the posterior margin. In the extra-fulcral portion the furrow occupies nearly the whole width of the pleura. The free ends of the pleuræ are truncate.

The pygidium, which is about one-third the length of the head-shield, is rounded and transversely expanded (being nearly three times as broad as long), and has a moderately convex axis occupying in breadth about one-fifth of the surface. This axis tapers slightly to an obtuse end which almost touches the posterior margin, and is crossed by three or four faint annulations. Two to four indistinct grooves are visible on the lateral lobes of the pygidium, the broad posterior border of which is sharply bent down.

Affinity.—The figures in Angelin's "Palæontologia Scandinavica" (*Trilobita*, t. xli. figs. 1, 1*a*, *b*, *c*, p. 84) of *Trinucleus bucculentus* (Ang.), if they had been supplemented by a more minute description, would have enabled us to institute a more exact comparison than is now possible; for undoubtedly the Irish form approaches more

closely to this species than to any other. The points of agreement with Angelin's species are (1) the general appearance and amount of inflation of the parts of the head-shield; (2) the shape of the glabella and its lobes; (3) the tubercles and ornamentation on the glabella and the cheeks; (4) the presence of radial sulci; (5) the general characters of the pygidium. The peculiarities of the Irish form, and its differences from the foregoing species, are (1) the non-projection of the glabella beyond the fringe; (2) the shape of the cheeks and their lack of any lateral overhang of the fringe; (3) the minute size and relations of the sulci, and the pits in them; (4) the outward curvature of the spines.

The lower surface of the fringe and the thoracic rings are not figured by Angelin, but the Irish form has in them features not found in other figured or described species. Under these circumstances the Irish form deserves to rank, for the present at any rate, as a distinct species, and it may be called after its home, *T. hibernicus*.

EXPLANATION OF PLATE III.

Fig. 1. *Cybele tramorensis.* × 3.

Fig. 2. *Trinucleus hibernicus.* ♀ Head-shield nearly perfect, showing general shape and characters. Variety with many lateral sulci. The glabella is worn and imperfect. × 3.

Fig. 3. Ditto. Head-shield showing lobes of glabella, tubercles, etc. × 3.

Fig. 4. Ditto. ♂ Impression of thorax and of lower surface of fringe of rolled-up specimen. × 6.

Fig. 5. Ditto. Impression of head-shield of rolled-up specimen Fig. 4. × 3.

Fig. 6. Ditto. Portion of upper surface of fringe, showing pits in median sulci. × 6.

Fig. 7. Ditto. Reticulate ornamentation of glabella and cheeks. × 18.

II.—A Criticism of the Astronomical Theory of the Ice Age, and of Lord Kelvin's Suggestions in connection with a Genial Age at the Pole.

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(*Continued from the January Number, page* 13.)

(Part II.)

IN the previous portion of this paper, which appeared in the January Number, I examined the form in which Croll presented the astronomical theory of the Ice Age and endeavoured to show how absolutely unsound his argument is. It is, however, at least as necessary to discuss the form in which Sir Robert Ball presents it; for much of the recent success of the theory, outside the school of modern geologists, was due to the weight naturally attached to the fact that a writer of Ball's scientific eminence believed he had so materially strengthened Croll's astronomical argument, that had Croll himself been aware of its full force, he would not have felt bound to call in such auxiliary agencies as a diversion of the Gulf Stream from its course. If, then, the theory, as presented by Croll, was able to win its way with such success among scientific authorities, it must surely be irresistible in the new and more powerful form. But it will be seen that, so far from strengthening Croll's position, Ball's statement weakens it very materially.

I. Examination of Sir Robert Ball's "Cause of an Ice Age."

The first pages which deal with the astronomical explanation of the Ice Age are pp. 43–47. Here Ball practically agrees with Croll; the only difference being that while Croll takes — 239° F. as the "natural zero," or temperature, to which the earth would fall if the sun-heat were removed, Ball takes —300 F. Thus, all the objections to Croll's argument C (see the paper in the January Number) apply equally here. In addition to what I have there pointed out, I give the following discussion to show how the method on which Ball and Croll rely, if properly followed, confirms my contention.

Let us try to apply this method to the temperature of Great Britain, amending it where it is in direct conflict with known physical laws. We must proceed somewhat as follows: Since the temperature reaches its maximum and minimum in July and January respectively, we know that in each of those months the total heat received is equal to the total heat lost. Now, the loss arises from two causes—first, heat lost by radiation from the earth; and second, heat lost by transference to other parts of the globe. Similarly the gain arises from radiation from the sun, and from heat transferred from other parts of the earth. We can strike a balance between the heat lost and gained by transference, and call it the heat gained by transference; but we must, of course, remember that this will be a negative quantity if more heat be transferred northward than is received from southward.

Now we assume with Croll that the radiation from the surface of the earth is to be treated as a direct radiation into space; that is to say, we neglect what is probably one of the most important factors in terrestrial temperatures, the blanketing effect of the atmosphere. Then, assuming the truth of Stefan's law, we suppose the loss by radiation to be proportional to $(460+t)^4$. Hence, if H be the heat transferred, and S the sun-heat and stellar radiation received, then since in January and July, when the temperature is stationary, the heat received must equal the heat lost, we get the equation,

$$H + S = A(460 + t)^4$$

where A is the coefficient of radiation. As we do not know the value of H, we must make various assumptions and see what their effect will be. First, suppose that in July $H = 0$. Then, taking lat. 55° as representing Great Britain generally, we have for July $S = 1700$, and $t = 60°$ F., and for January $S = 265$ and $t = 40°$ F., the 1700 and 265 being, of course, on an entirely arbitrary scale. Hence for July,

$$1700 = A(460 + 60)^4$$

which gives

$$A = \frac{1700}{(520)^4}$$

Hence for January,

$$H + 265 = \frac{1700}{(520)^4}(500)^4$$
$$= 1455$$

This gives for H, the heat transferred to us from the Gulf Stream, etc., in January, the equation

$$H = 1455 - 265 = 1190$$

Now, in order to get the direct effect of diminished sun-heat, let us suppose that the heat transferred in January in the epoch of great eccentricity is the same as that now transferred, and we get for the temperature at that period the equation

$$1190 + S' = \frac{1700}{(520)^4}(460 + t)^4$$

where S' is the reduced sun-heat radiation in the epoch of great eccentricity. But the sun-heat is reduced by about 16 per cent., so that if we neglect the star-heat we get for S' the value 223. Hence the equation becomes

$$1190 + 223 = \frac{1700}{(520)^4}(460 + t)^4$$

and this gives for t a value between 36° F. and 37° F. Hence, on this supposition the fall in temperature would be between 3° F. and 4° F., about the same as that obtained by my much safer method of comparison.

If we suppose that in July we receive a balance of heat from the Gulf Stream and air-currents, we should get a still smaller lowering of temperature in the supposed Glacial epoch. For instance, if H, the July heat transferred, was equal to the July sun-heat, then the above fall in temperature would be reduced by almost exactly one-half. If, on the other hand, we lose half our summer sun-heat by transference to more northern latitudes, then the fall in temperature would be about 7°, nearly double that obtained on the first supposition.

I do not give this as a safe method, because in the first place it neglects the effect of the *blanketing* by the atmosphere; in the second place there is the uncertainty about H, and in the third place although it is quite certain that the radiation increases faster than the temperature, yet Stefan's law may not be strictly applicable to the radiation of the earth in space. I insert it merely to show that a little physical knowledge of the problem they were dealing with would have saved the astronomical geologists from making such extraordinarily mistaken estimates of the effect of the decreased sun-heat on terrestrial temperature.

The fact that the lowest temperatures occur almost simultaneously about January all over the globe, as shown by the "Challenger" isothermal maps (just as the highest temperatures occur about July), indicates that the upper regions of the atmosphere have a very great controlling influence on the loss of heat by radiation, and that instead of considering that the earth radiates heat off to space it would be more correct to regard it as radiating to those high altitudes of atmosphere in which, for two reasons, the temperature is probably much more uniform between the poles and the equator

than it is nearer the earth's surface. For in the first place, there is probably a very rapid interchange of air between the poles and the equator, and in the second place, the upper strata of the atmosphere are warmed by the sun with far more constancy than the lower strata; even at the earth's surface twilight lasts till the sun is 18° below the horizon, and as we ascend the horizon is depressed. If it were not for this, we should expect the polar regions to go on cooling down during all their long night of winter.

From p. 48 we pass to p. 78, where the exposition of the theory really begins. There is nothing in the well-known theorems here set forth which calls for comment till p. 89, where I find the first indication of a statement, repeated more than once later on, to the effect that Dr. Croll was under the erroneous impression that the winter sun-heat was equal to the summer sun-heat, the difference in temperature arising only from the greater length of the winter. It is due to Dr. Croll that this misconception should be removed, and I am sure Sir Robert will be the first to acknowledge the justice of the correction. In "The Cause of an Ice Age," p. 89, we read: "Do not read this as if it asserted that the heat received during summer is equal to that received during winter. This is not true, but it has been often asserted. It is, in fact, the fundamental error which has vitiated the astronomical theory of the Ice Age *as previously presented*, and which it is the chief object of this little book to expose."

Again, p. 115: "Not only did I find that the true law had been overlooked but it became apparent that sometimes another law had been adopted which was absolutely incorrect. I found, furthermore, that the astronomical theory of the Ice Age as ordinarily stated was contaminated with absolute blunders in the simple mathematical questions which are involved."

Having then quoted what is certainly either a careless mistake or a mere oversight of Herschel, who incidentally states that half the heat of the year is received in summer and half in winter, Sir Robert proceeds (p. 118): "Indeed, on such a matter it was not unnatural that the words of Herschel should have been adopted without any very careful scrutiny. *I examined Croll's book carefully on this point.* . . . . So far as I can gather from the various allusions to the astronomical element of the problem in his writings, *it would seem as if he entertained the same view as that which is incorrectly set down in the 'Outlines of Astronomy.'*" Again, p. 135: "He [Dr. Croll] appears, in common with many others, to have been influenced by Herschel's unfortunate inadvertence."

I had read Croll's "Climate and Time" before I read "The Cause of an Ice Age," and I certainly had not received the impression that Croll was under the misconception supposed by Ball. Indeed, it seemed to me a thing incredible that anyone should *really* suppose the winter sun-heat at, say, the North Pole, was equal to the summer sun-heat at the same place. Accordingly I examined Croll, and I cannot find the slightest ground for the idea that Croll entertained the erroneous view referred to; but, on the contrary, he

more than once lays down the law in the correct form in which Ball states it, and in p. 87 of "Climate and Time" Croll incidentally shows how well aware he was of the true nature of the distribution of annual heat. Speaking of the latitude of Edinburgh, he says: "The quantity of heat received during winter is *scarcely one-third* of that received during summer," and further down on the same page he speaks of the "*deficiency of heat received in winter from that received in summer.*"

In the first of these quotations from Croll we see one great difference between his method and Ball's, a difference in which the advantage seems to me to lie wholly with Croll. For, while Croll carefully bears in mind the different relations which exist between the winter and summer sun-heats in different latitudes, Ball *lumps together the winter sun-heat over the entire northern hemisphere,* and compares it with the summer sun-heat over the *entire* hemisphere. He does not suggest any *reason* for introducing the sun-heat over the tropics in a question which relates only to the temperature of the northern latitudes. He does not seem to think it necessary in any way to justify or explain the application of the numbers 63 and 37 (which, of course, do give the relative summer and winter sun-heats received over the entire of either hemisphere); he merely *states* that it is impossible to discuss the astronomical theory of the Ice Age unless these figures form the refrain of every argument (pp. 90, 91). But in fact these figures, 63 and 37, have little or no relation to the astronomical theory of the Ice Age, which is really the theory of the temperature of the northern latitudes in the winter of great eccentricity. For the temperature at any latitude is determined by four factors—

(1) The gain of heat transferred to it from warmer latitudes by means of air and ocean currents.

(2) The loss by heat transferred from it to colder latitudes by air and ocean currents.

(3) The sun-heat received by it.

(4) The heat radiated by it into space.

Now neither the summer nor winter value of any one of these quantities is represented by, or proportional to, the numbers 63 and 37. They vary from latitude to latitude, and it would be impossible for any single pair of numbers to represent the combined effect of the very complicated quantities over the different latitudes to which the theory of the Ice Age must be applied.

But if we are to take two numbers to represent the summer and winter sun-heats which determine the temperature of the glaciated regions, we should at least take figures which bear some relation to the sun-heats which actually fell on those regions. Now, since considerably more than half the annual sun-heat falls between the equator and latitude 30° (as is evident from the fact that sin 30° $=\frac{1}{2}$), and since this amount is not far from equally divided between summer and winter, it follows that out of 100 units of heat falling annually on the northern hemisphere, say 25 falls in summer and 25 in winter within latitude 30°, leaving only about 63—25 and

37—25, or 38 and 12, for the summer and winter sun-heats on the portion of the hemisphere north of latitude 30°. This rough and ready calculation is made merely *to show how far the numbers 63 and 37 are from any numbers which could be supposed to give the ratios of the summer and winter sun-heats in those regions of the earth with whose temperature during the long winter of great eccentricity it is the business of the astronomical theory to deal.* The actual percentage of summer and winter sun-heat are—for lat. 50°, 73 and 27 (nearly 3 to 1); for lat. 60°, 82 and 18 (more than 4 to 1); and for lat. 70°, 91 and 9 (more than 10 to 1). How greatly must the astronomical theory of the Ice Age, which is neither more nor less than the theory of the temperature of these latitudes during the long winter of great eccentricity, be weakened by substituting for these numbers the uniform ratio of 63 to 37!

Again, the mode in which Sir Robert uses these figures 63 and 37 to arouse in the untrained imagination the picture of a winter of appalling severity is very unsafe. The argument is put on p. 135. If in the period of great eccentricity "the [nearly] double supply of heat [63 measures] be poured in like a torrent during the short season [166 days], while the single supply of heat [37 measures] is constrained to do duty over the long season [199 days], then an intolerable climate is the result. The total quantity of heat received on the hemisphere in the course of a year is no doubt the same in each case; but its unsuitable distribution bespeaks a climate of appalling severity—an Ice Age, in fact."

It will probably come as a surprise to most readers of this passage to learn that they are even now suffering under a still more intolerable climate than this, one of far more "appalling severity." In the latitude of Dublin, where the book was written, while 63 measures of heat were poured in like a torrent during the 166 hottest days of the year, not 37 but only 22 measures of heat are left to do duty over the long period of the 199 coldest days; that is, *less than two-thirds of the amount which is supposed to produce an Ice Age.* In fact, we have to go as far south as Madrid, Naples, or Constantinople, in lat. 41°, to get the summer and winter distribution of sun-heat during the 166 and 199 days in the appalling proportions of 63 and 37.

There are two other quantitative statements relative to alteration in temperature made by Ball. The clearest is on pp. 130, 131, where he deduces the result of a method of using the numbers 63 and 37 to obtain a numerical statement. He finds that in the Ice Age the mean annual range of temperature of Great Britain will be increased from its present value of 20° F. to 28° F. Surely this is a *reductio ad absurdum* of the theory. There is hardly a climate in the northern hemisphere where the range is not much greater than 28° F. In latitudes 40°, 50°, and 60°, in America, the ranges are 50° F., 65° F., and 75° F. respectively, but we have no Ice Age there. Why should a range of 28° F. in Great Britain be supposed to produce an Ice Age? The reason given is (p. 131): "It is to be observed that, generally speaking, the coldest places

are those of the greatest mean annual range. We are, therefore, *entitled to infer* that the effect of such a change in the eccentricity as we have supposed would be to increase the range, lower the temperature of the hemisphere, and thus induce the Glacial period." How are we to discuss such an assertion? It amounts to this, that *without examining what the alleged connection between annual temperature and annual range is,* and *without any previous attempt to ascertain what temperature is necessary to bring about an Ice Age,* we are "entitled to infer" that a range of 28° F. in Great Britain would induce the Glacial period! So far from that being the case, the most cursory examination of the far greater ranges of temperature in non-glaciated regions at present seems to entitle us to infer that the supposed increase of range would be quite powerless to effect the desired, or indeed, any very marked change in climate.

The other quantitative statement is to be found on p. 105, where, still working on the basis of the 63 and 37, Ball calculates that in the Glacial winter there would be a deficiency of 7 per cent. in the average daily supply of sun-heat as compared with our present winter supply, the unit being the average annual daily sun-heat. As the unit, according to Ball's supposition, keeps Great Britain 350 degrees above the natural zero, this might be supposed (according to the erroneous theory adopted by Ball and Croll alike) to produce a lowering of 24·5° F. in the winter temperature of Great Britain. Ball does not here make the calculation, but I make it on his principles in order to show how small is the result obtained by Ball as compared with that of Croll, who gets a lowering of 45·3° F. in the winter temperature of Great Britain. Hence, so far from Ball being correct in thinking that if Croll had known his form of the purely astronomical portion of the theory, he would not have considered the alteration of the Gulf Stream a necessary adjunct, it would seem as if Croll would then have wanted a second Gulf Stream to help him for the 45·3 *minus* 24·5 degrees of refrigeration he would have to abandon.

I now pass to another point. It was long ago stated by Tyndall that great heat to produce evaporation was as necessary in giving rise to a Glacial age as great cold to produce condensation. After agreeing with this, Ball states that the astronomical theory shows where to look for the necessary heat. He points out correctly that the annual quantity of heat received by the whole earth is not lessened, and he continues (p. 109)—"If, therefore, the glaciated hemisphere received during its winter a deficient supply of heat, to which the glaciation owes its origin, *the defect of heat upon this hemisphere will have to be compensated for elsewhere.* More favoured regions of the globe will accordingly enjoy a more copious supply of sunbeams, and thus those volumes of aqueous vapour will be provided which, put in circulation by the winds, are deposited in the form of snow when the ice-sheets are growing." Here is an oversight, not unlike that of Herschel, to which reference has been made. The sentence I have italicised should read, "the defect of *winter* heat on this hemisphere is compensated for," not else*where*,

but else*when*, *i.e.* "in summer on the same hemisphere." The "more favoured regions" are enjoying a *cooler* summer than usual, for, in the Glacial winter the whole earth is further from the sun than usual, and every region receives less heat. Hence it follows that if the extra heat of the short Glacial summer is to produce the increased evaporation, and if the snowfall is to be in winter, it is necessary that the moisture thus evaporated in summer shall be held in the atmosphere till precipitated by the cold of winter. Croll, who saw well the difficulty here involved, boldly asserts that the chief snowfall will be in summer.

I have now disposed of every argument of importance put forward by Ball, and shown that the theory as presented by him does not even *claim* to account for at all as great an effect as that asserted by Croll. For, as has been shown above, Croll claims a lowering of 45·3° F. in the mid-winter temperature of Great Britain, while Ball's theory only gives about 25°. Hence it is quite a mistake to suppose that though Croll's mode of presenting the astronomical part of the theory may require to be strengthened by considering the changes produced by ocean currents, such auxiliary causes are rendered unnecessary by the changes Ball has introduced. Evidently they would be more necessary on Ball's theory than on Croll's.

Hence the net result of the discussion is that the theory remains where Croll left it.

In concluding a long and somewhat dry criticism, I may be allowed to express my satisfaction in finding that the conclusion I have reached on purely physical grounds, negative though it be, is so far in accord with the views of the modern school of geologists that it permits of the date of the prehistoric glaciations being fixed in accordance with the geological evidence, and, as I venture to think, finally removes a restriction as to the date of the Ice Age, which many geologists had felt to be an unreal one.

[Since writing this paper, my attention has been directed to Sir H. H. Howorth's *Glacial Nightmare*, in which there is a long and able criticism of the Astronomical Theory of the Ice Age, in which a summary of the criticisms advanced by previous writers is included. The chief feature of this paper, *i.e.* the mode of calculating a limit to the probable alteration in terrestrial temperatures due to direct sun-heat, is not of course referred to, not having been published until last year.]

As to the genial age, we have the great authority of Lord Kelvin for thinking that it might be due to a more open Arctic Sea in which a great influx of warm water was maintained. See *Geological Climate* in his recently republished *Lectures and Addresses*.

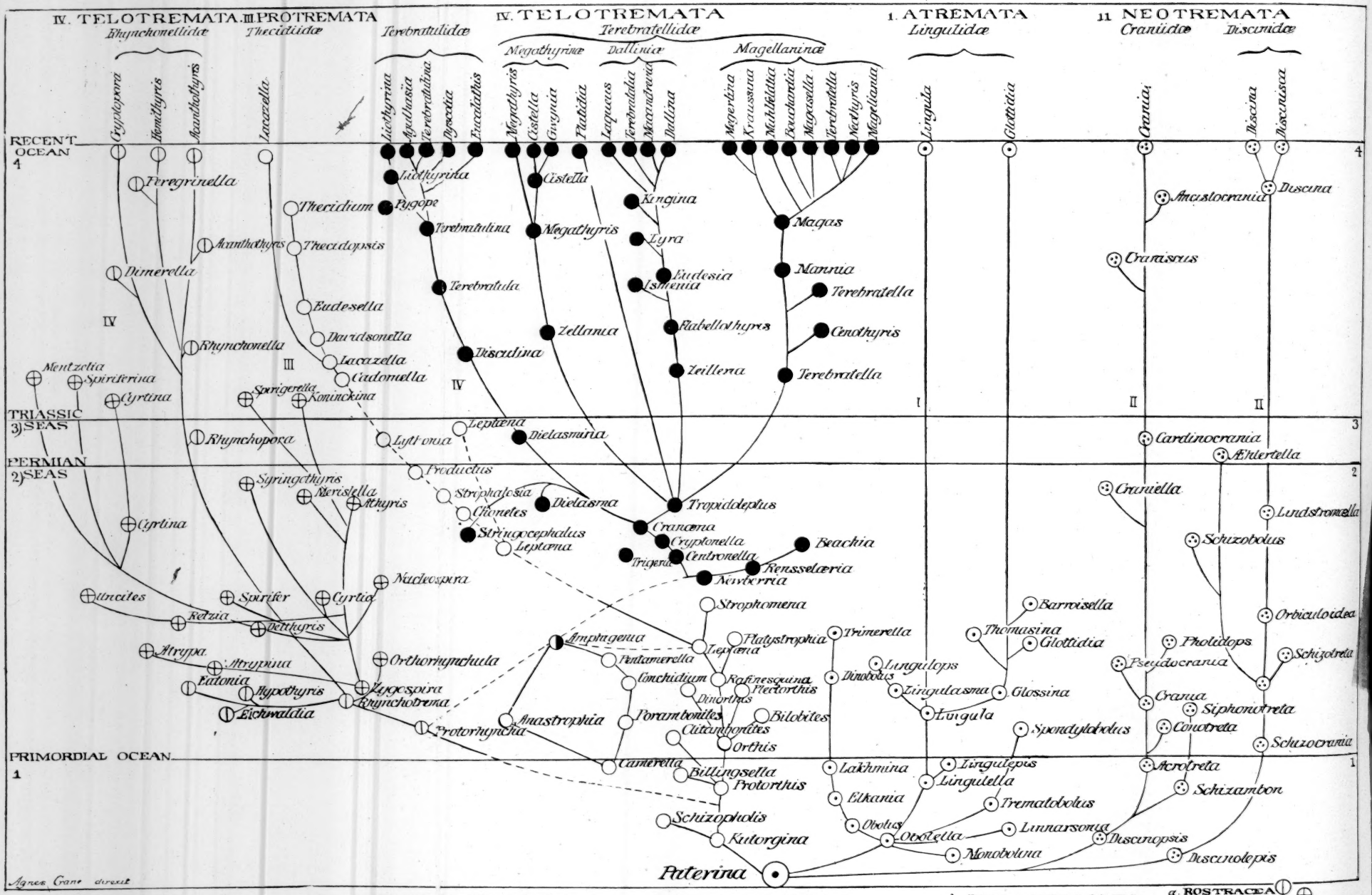
After saying that a sinking of 1000 feet would submerge vast areas of land, and leave the North Pole very free and open to access by sea, he continues: "Just think of a current of three-quarters of a nautical mile per hour, or 70 miles in 4 days, 40 fathoms deep, flowing across the Arctic circle to the Pole. This would cover the whole area by one fathom, and if 7·1° C. above the freezing-point would

bring in enough heat to prevent freezing, if in 24 hours as much heat were radiated away, as, taken from 7·2 inches of water at zero, would transform it to ice. This is no doubt much more than the actual radiation would be if the water were 0°C. at the Pole and 7·1° at the Arctic circle." Add to this a south wind at 15 miles per hour through 2300 feet above the surface, and also at 7·1° C. (which would carry as much heat as 15 fathoms of the water), and we have the agencies suggested by Lord Kelvin in 1877 for the production of a genial period at the Pole. But suppose we grant that they would keep the Arctic circle from frost, is there the least probability of such agencies ever coming into play? The Gulf Stream flows at less than 4 miles per day, and we are asked to suppose a current flowing at 18 miles per day! Where is the impelling force to come from? Apparently Lord Kelvin suggested that the difference in specific gravity due to temperature might be the motive power, but in view of the final abandonment of temperature differences as causes of ocean currents on a large scale, this cannot be admitted as sufficient to produce a current of anything like the required depth or velocity, and whether we assign winds or difference of specific gravity as the cause, we are met by the difficulty that, as we have ultimately to depend on differences of temperature in different latitudes, Lord Kelvin's result seems inconsistent with his cause, for he supposes the temperature differences to be almost annihilated. Again, in January, we have to go far south of the Arctic circle to get a temperature of 7·1° C. (nearly 45° F.). Again, if the forest trees of Greenland, Grinnell Land, and Spitzbergen were only found at a height of 1000 feet above the present sea-level, it might be rational enough to account for the high temperature of which they are the evidences by supposing a submergence of the land to that extent, but, since this is not the case, we cannot postulate such a submergence to explain the phenomena.

As to an explanation of prehistoric glaciation, I am afraid I have nothing very definite to offer. Of course an alteration of altitude is sufficient, but the geological requirements do not seem to admit of a very free use of this method of explanation.

If, indeed, we could see any probability that at a former age the south winds passed in the upper regions of the atmosphere in summer, as they now do in winter when the surface wind is from the north, so that there was a continuous and permanent north wind blowing across the Arctic circle down to temperate latitudes, then we might find a full explanation of an ice-age gradually spreading southward over the land, especially as such winds would either impede or reverse the Gulf Stream. I am not at present, however, able to suggest any cause for such a difference from the present distribution of the winds.

It is not unlikely that there never was an Ice Age, but that there have been at times various local glaciations such as we now see in Greenland. The present glaciation in Greenland is supposed to be in great part due to the cold-water current which flows along its shores from the Polar regions, and this, in its turn, is in part due



AN ATTEMPT TO INDICATE THE GENERIC AND ORDINAL EVOLUTION OF THE BRACHIOPODA.

this supposition is, that the distribution throughout interstellar space is not permanent and does not follow the error law. If two bodies flying about in space come into collision they may generate a mass of gas, and if this mass of gas happens to get in the earth's path it may be caught up. Indeed, if we assume that the earth's atmosphere maintained a strict equilibrium with the interstellar molecules at, say, a distance of 1000 miles from the earth's surface, then a doubling of the almost infinitesimal pressure there, would necessitate a doubling of the pressure at the earth's surface. Of course this supposition is only used for the purpose of illustrating the fact that a small alteration of interstellar pressure, if spread over a sufficiently vast space, might eventually give rise to a considerable change in the atmosphere. Moreover, when considering the applicability of Mr. Bryan's investigations, we must not forget the enormous speed with which the earth meets the interstellar gases. Hardly any molecules of those gases can be travelling at anything like the speed with which the atmosphere charges into them. When in addition I remember that the atmosphere is in a state of rotation, during this charge into the interstellar matter, I comfort myself with the thought that the problem of atmospheric interchanges is so complicated that I may at all events hope to enjoy my hypothesis for a considerable time before anyone succeeds in giving it a really decisive overthrow. Its great advantage as a theory of the Glacial epoch is, that it does not require geographical changes such as are usually postulated in connection with all other theories, even the Astronomical one.

III.—The Evolution of the Brachiopoda.[1]

[A Sequel to Dr. Thomas Davidson's "What is a Brachiopod?"][2]

By Agnes Crane.

(PLATE IV.)

Part I.

FOR many years I have been deeply interested in the general history of the Brachiopoda, and I have enjoyed the acquaintance, correspondence, and friendly encouragement of most of the eminent biologists and palæontologists who have devoted themselves to the elucidation of the complex structure of this class. Here I must name with reverence Davidson, long my friend and master; Barrande, of Prague; James Hall, of Albany; William King; Eugène Deslongchamps, the Norman naturalist; Suess, of Vienna; Friele, of Bergen; Morse, of Salem; and Dall, of Washington; "all honourable men," and many others of the *fin du siècle* school, whose names will be noted in the sequel.

During this period the history of the class has been completely re-written. The Brachiopoda now seem to justify the prescience of

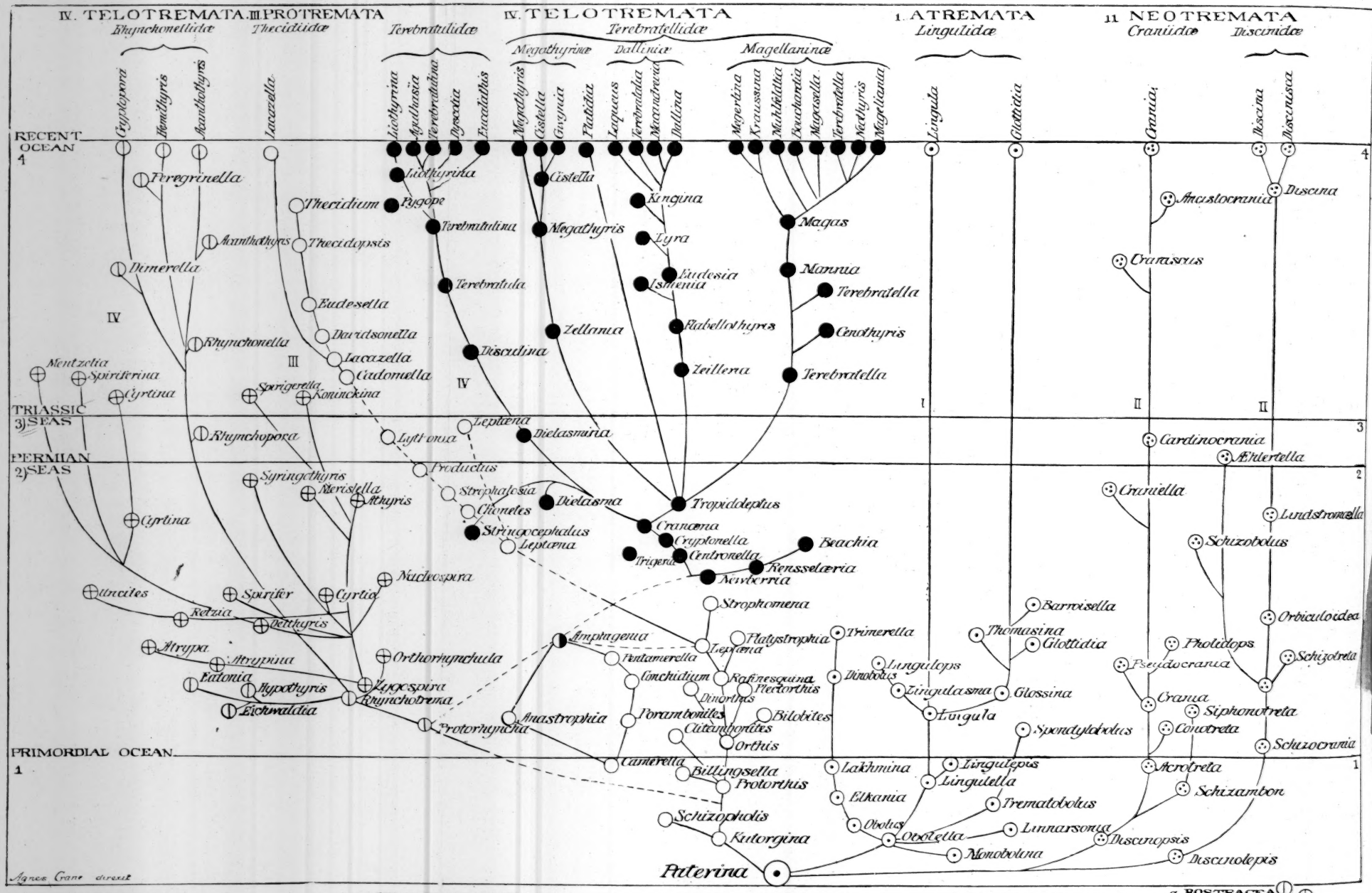
[1] Read at Chicago, August 24, 1893, before the Women's Auxiliary Branch of the World's Congress (Section Geology), Department of Science and Philosophy; also before the Brighton Natural History and Philosophical Society, November 13, 1893.

[2] Geol. Mag. 1877, Dec. II. Vol. IV. pp. 145–155, and 199–208.

to the entry into those regions of the Gulf Stream, so that the glaciation of Greenland is the counterpart of our genial climate. Into these geographical conditions I do not, however, propose to enter here; I will content myself with a general argument for the geographical theory of glaciation as opposed to the astronomical, namely, that whereas the change in the winter isothermals due to sun-heat in times of great eccentricity amounts at most to about 4° of latitude, the present changes in winter isothermals amount to over 33°, as where the 0°C. winter isothermal passes from south of New York to north of the North Cape, a shift due to geographical conditions about eight times as great as any shift due to astronomical conditions could be.

Is it possible that there may have been any considerable interchanges of atmosphere between the earth and the regions of space through which it has passed? It is certain that there must have been some such interchange. Whether the atmospheric pressure is increasing or diminishing depends on whether, in the course of the earth's motion through space, more of the interstellar molecules get entangled in the earth's atmosphere than, leaving that atmosphere, get entangled in the interstellar gases through which the earth happens to be passing, and are thus dragged away from the earth. If once we were allowed to assume the magnitude of these changes, the whole difficulty of explaining Glacial or genial ages, so far as temperature changes are concerned, would vanish. For instance, if due to some gaseous conditions of space, or perhaps to the absorption into the atmosphere of the gaseous components of meteorites or shooting stars, there be an addition to the atmospheric pressure of one millimetre in three centuries, and if this process has been continued for 20,000 or 25,000 years, then it follows that, some 25,000 years ago the atmospheric pressure would have been less by about one-tenth part than it is at present. This would be equivalent to raising land and sea by about 2500 feet, for the blanketing effect of the decreased atmosphere would be about the same as that which now lies above a mountain 2500 feet high. Thus a Glacial epoch might be produced *without any alteration in the geographical conditions.* And if, on the other hand, either through the earth plunging into a more gaseated region of space, or through some catastrophe, the atmospheric pressure were to be much increased, the resulting increase of temperature might be very great, and a genial age might be the result—a rise of 50° F. might readily be got.

No doubt there is much that may fairly be called "vague speculation" in this suggestion; but there is an advantage even in that, for the more vague it is the more difficult to refute it. Certainly I do not think it can be refuted by calculations such as those of Mr. G. H. Bryan, p. 682 of the 1893 Report of the Brit. Assoc., on the supposition that the temperature is constant and that the gases have established themselves in a permanent state. Thus he finds that at any given instant there are very few particles ready to leave the atmosphere of a large planet. But the very essence of



I. ATREMATA ⊙ II. NEOTREMATA. ⊙. III. PROTREMATA. ○ IV. TELOTREMATA

AN ATTEMPT TO INDICATE THE GENERIC AND ORDINAL EVOLUTION OF THE BRACHIOPODA.

this supposition is, that the distribution throughout interstellar space is not permanent and does not follow the error law. If two bodies flying about in space come into collision they may generate a mass of gas, and if this mass of gas happens to get in the earth's path it may be caught up. Indeed, if we assume that the earth's atmosphere maintained a strict equilibrium with the interstellar molecules at, say, a distance of 1000 miles from the earth's surface, then a doubling of the almost infinitesimal pressure there, would necessitate a doubling of the pressure at the earth's surface. Of course this supposition is only used for the purpose of illustrating the fact that a small alteration of interstellar pressure, if spread over a sufficiently vast space, might eventually give rise to a considerable change in the atmosphere. Moreover, when considering the applicability of Mr. Bryan's investigations, we must not forget the enormous speed with which the earth meets the interstellar gases. Hardly any molecules of those gases can be travelling at anything like the speed with which the atmosphere charges into them. When in addition I remember that the atmosphere is in a state of rotation, during this charge into the interstellar matter, I comfort myself with the thought that the problem of atmospheric interchanges is so complicated that I may at all events hope to enjoy my hypothesis for a considerable time before anyone succeeds in giving it a really decisive overthrow. Its great advantage as a theory of the Glacial epoch is, that it does not require geographical changes such as are usually postulated in connection with all other theories, even the Astronomical one.

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Darwin. Formerly regarded as one of the most obstinate difficulties in the way of the demonstration of the evolution of invertebrate life on earth, they now bid fair to become a remarkable illustration in favour of it. It is no longer possible to doubt that the life-history of the Brachiopoda does yield convincing testimony of the truth of the law of Evolution, and to the establishment of this fact American scientists have largely contributed. Their work is the more creditable to them because so often carried on amidst a constant fight for "appropriations" and a liability to upset from the exigencies of the political situation—trials happily unknown to our more favoured scientists. In the Western world the minds of scientific workers in general seemed more favourable to the acceptance of the great principles of Evolution conceived and first promulgated in Europe. In America they sought rather to prove the argument than to controvert it. They were, perhaps, less blinded by that third eyelid "the nictitating membrane" which their genial and witty philosopher, Wendell Holmes, has declared to be common alike "to reptiles, some birds, and theological students," and by means of which, he said, "they shut out," not all the light, "but all the light they do not *want!*" The genus is not absolutely unknown in this country, although more rare than formerly. It is to American biologists that we owe not only the systematic correlation among invertebrates of the development of the individual (ontogeny) with the phylogeny or evolution of the group to which it belongs, but also the important discovery of the radicals of many classes of molluscan animals. Such ancestral root-stocks have been determined by Hyatt for the Cephalopoda, by Jackson for Pelecypoda (in *Nucula*), and by Beecher for the Brachiopoda. The radical of the Brachiopoda is the Lower Cambrian genus the father-like *Paterina*, of which more anon.

The Brachiopoda, it is well known, have persisted through all the life epochs of the geological past. Their bivalve shells occur fossil in all marine deposits ranging from the Primordial to the Quaternary. They were, however, much more abundant during the "old time" and medial ages of geological history than during these "latter days" of the earth. Out of the 277 described genera 186, or two-thirds of the whole number, appeared in Palæozoic seas, and a goodly number were evolved during the great Secondary epoch.

Thousands of species or "mutations" have existed in the past, but now they can be numbered by tens instead of by thousands, for there are fewer surviving species than there were genera in the Palæozoic era. The actual number of living species was estimated by Davidson—in that posthumous "Monograph on the Recent Brachiopoda" which it became my sad duty to edit for the Transactions of the Linnæan Society—at 99 certain, and 29 doubtful species, referable to 22 genera and subgenera. With the modern tendency to generic subdivision and stricter revision of species it is probable that this estimate will be reduced rather than augmented, so far as the number of species is concerned (27).[1]

[1] These numbers, in parentheses, refer to the Bibliography given at the end of Part II.—Edit. G.M.

The living forms are universally distributed in the seas of the world. Their range in depth is no less extended. They occur in shallow waters, at low water-mark, and varying degrees of depth, from 200 to 600 fathoms being the usual limit of the majority of species. Several far-ranging abyssal species were dredged in from 1000 to 2000 fathoms. The delicate transparent shell of that interesting little Terebratuloid *Liothyrina* (*Terebratula*) *Wyvillei*, Dav., was actually obtained in a living condition by the "Challenger" Expedition from the enormous depth of 2900 fathoms, or three miles and a quarter, at the bottom of the South Atlantic Ocean (30).

Such manifest adaptability to varied environments is probably one of the main causes of the persistence of the group throughout the geological ages. The Brachiopoda seem to flourish alike in tropical seas and the frigid abysses of the ocean. They are extremely tenacious of life in captivity, pursuing—

"The noiseless tenour of their way"

for months and even years together. An occasional yawn seems to be their only diversion. The animals are, however, sensible to light and close their valves abruptly on the interposition of any shadow. The "gape" is less than in the oyster. Civilized man who swallows that succulent, but often insanitary, bivalve, is apt to draw the gastronomical line at the Brachiopod, but in the Philippine Islands where Lingulæ abound and are cast up by the sea, like mussels on our shores, the natives collect them for food. It is evident from the small space alloted to the animal in some of the fossil species that there was very little to eat in them. Many fishes are ardent, if somewhat indiscriminating, "collectors" of Brachiopods. One would imagine the spinose forms would prove rather indigestible; but the contemporary sharks and other fishes were furnished with powerful crushing teeth, and doubtless made "small bones" of those ornamental appendages.

The use of these spines, so extensively developed in adult individuals of several families, is not actually determined. In some species they were evidently flexible, and served as moorings or as clasping organs, as in the case of that little Productoid which Fischer and Œhlert re-named *Etheridgina* in honour of its first describer, for this *Productus complectens* clasped its spines round the stems of the sea lilies, swaying to and fro in the ocean's depths. In other forms they were tubular, and Dr. John Young has described species with double spines, one contained within the other (74). Such may have subserved the function of respiration, or to admit water to the interior of the shell preventing the introduction of impurities, as De Verneuil had previously maintained. The spines obviously afforded no protection to the species in the struggle for existence, for only one spinose form survives out of many members of various families thus ornamented. This interesting example is that of the little living Rhynchonelloid which was dredged by Döderlein in Sagami Bay, Japan, and was named *R. Döderleini* by Davidson just before his death (31). This curious survival belongs

to that sub-section of spiny Rhynchonelloids denominated *Acanthothyris* by D'Orbigny, so abundantly represented in the Oolitic seas. That acute Norman palæontologist, Eugène Deslongchamps, however, considered it more closely reproduced the fashion of spinose ornamentation, characteristic of certain Palæozoic genera, as the spines are arranged in regular rows at intervals, as in *Atrypa*, a Palæozoic form, and are not irregularly dispersed over the surface, as in the spinose Rhynchonellæ of the Jurassic epoch (34).

There are, indeed, "all sorts and conditions" of Brachiopoda; they vary in size from a pin's head to nearly a foot in length and in breadth (*Productus giganteus*), from the Carboniferous Limestone. Their familiar forms present so many queer shapes and ornaments that it is impossible now to detail their external features.

But something must be said with regard to the animal inhabiting the shells, for the days of the simple conchologist, content with mere externals, have passed away—or he survives only for the benefit of the dealer. The study of what was scornfully termed the "nasty animal" has become one of the most important conditions of biological research from the earliest free-swimming larval phases to the adult fixed condition of the individual. The embryologist now follows the life-history of a Brachiopod from the ova through all its stages of development. The bathmologist watches it from the first moment of the growth of the *protegulum*, or initial shell-covering, through infancy to youth, from youth to adolescence, from adolescence to maturity, from maturity to decline, and from decline to absolute senility of extreme old age, when the venerable Brachiopod, like more exalted organisms, again reverts to its infantile characteristics. Then, alas! it sheds its surface ornaments, becomes bald, so to speak, and may even become obese!

It is to the recognition of the value of these methods of investigation, or the twin sciences of embryology and auxology, or bathmology, as Hyatt prefers to term it, inaugurated by Heckel, of Jena, extended by Hyatt, of Boston, and systematically applied to the Brachiopoda by Beecher and Clarke, that in a measure we owe the revolution of thought concerning the evolution of the Brachiopoda.

The class is again subdivided into two sub-classes distinguished by certain divergent features of both shells and animals, yet possessing some points of the nervous, circulatory, muscular, and reproductive systems in common. The animal in both sub-classes —developed from ova discharged from the fringed margins of the mantle into the sea-water—begins life as an active free-swimming ciliated embryo. It develops "eye-spots" and a peduncle by which in many species it becomes attached by its hinder body-segment, henceforth adopting a sedentary life. The shell, which is secreted by the muscular "mantle" envelope, then makes it appearance.

Brachiopoda belonging to the simpler and somewhat older type are characterized by hingeless and often impunctate shells in which phosphatic elements usually preponderate, with valves kept in place solely by shell-muscles. The "arms" or breathing organs are of a cartilaginous nature exclusively. The circulatory system is rudi-

mentary, the liver large, and the digestive canal has a terminal orifice, lateral in *Lingula*, dorsal to the median line in the anomalous *Crania*. The hingeless Brachiopoda are heartless creatures, and the circulation of the colourless lymph is effected by ciliary action. This group was named *Lyopomata*, or "loose valves," by Owen; *Inarticulata*, by Huxley; and King's name of *Tretenterata* indicates the important physiological distinction (48).

The *Arthropomata*, *Articulata*, and *Clistenterata*, on the other hand, are characterized—as their respective names imply—by jointed valves or articulated, essentially calcareous shells, with *hinged* valves united by interlocking hinge-teeth fitting into sockets in the opposite valve and in the majority by the absence of a visceral foramen. The cartilaginous arms are supported either by short bands or crura, by calcified loops, or spiral frameworks. The more highly developed members of the hinged sub-class have a lymphatic heart and supplementary vesicles (*Magellania venosa*), and a true vascular system. But accessory hearts are not uncommon in the animal kingdom. There is one situated, for instance, in the tail of the eel, which, perhaps, accounts for its extreme liveliness. In all the surviving species of this articulated sub-class the digestive tube terminates in a cœcum. Evolution does not always imply progress, however, and this is one instance of the origin of a sub-class through degeneration.

This peculiar physiological feature—a reversion to the simpler digestive process of the sea-anemone—is not predicable of the earliest fossil articulated Brachiopoda; for in some species of the protrematous and telotrematous orders the visceral foramen was retained. This visceral foramen was present in *Athyris*, *Renssellœria*, and *Cryptonella*, *i.e.* in members of the spire-bearing and Terebratuloid stocks at the earliest epoch of their derivation from the antecedent Protremata. Its closure was concurrently evolved no doubt with the gradual development of the interlocking hinged shell and subsequent growth of the shell-plates which ultimately covered in the deltidial area of the umbonal region. The change originated in that order Protremata, so prolific in short ranging genera. Is it unreasonable to assume that the period of this great functional derangement, a physiological crisis in the history of the development of the race, exercised an unfavourable influence on the vitality of the first articulated order in which it originated? It is a certain fact that only one genus out of more than eighty which have been referred to this order of Protremata have survived to the present day. The majority enjoyed but a comparatively short range in time; the sole survivor is *Lacazella Mediterranea*, characteristic of the secondary family of *Thecididæ*, and I am inclined to ascribe the persistence of this genus to the extraordinary development of internal calcareous spicula which permeate the mantle and form a protective covering to *all* the vital organs of the little animal inhabiting the shell—an absolutely unique feature among the Brachiopoda.

The class in general has otherwise progressed during evolution by expansion of the anterior elements of the body, development of

the heart and vascular system, and by the restriction of the pedicle opening to one valve of the shell. We can trace in the primitive members of the order Neotremata one manner in which this change originated. In the elongated forms of the non-perforated shells of the *Atremata* the long peduncle passes out freely between two valves. In some of the earlier forms a small notch is developed in that valve which becomes eventually the pedicle valve. In the primitive *Neotremata* this notch deepens, widens as in *Discinolepis*, and in *Orbiculoidea* and *Discina* it has become almost centrally situated, for the shell grew up later and enclosed the shorter byssus or mooring organ (Pl. V.[1] Figs. 12; 16 *a*, *b*, *c*; 20).

Hence a long peduncle is usually associated with elongated or oval shells, a solitary life, and free axial movement. A short byssus yielded less space to move in, and growth became concentrical. Thus the "disc shells" are co-ordinated with the short mooring organ. Both existing members of the "newly perforated" order (Neotremata) adhere in masses to each other or to rock-surfaces, and the consequent crowded condition of their environment affected the shape and growth-direction of the individuals. In *Crania*, which cling in clusters by their lower valves to rock-surfaces, the margins of the central shells cannot expand and the shell grows more in an upward direction; hence we get the conical helmet shells resembling a French cap of liberty. The divergence between the outer and inner shells of such a mass of individuals produces what some would consider as almost specific differences.

Such are some of the sad effects of overcrowding among the Brachiopoda. These serve as instances of the mechanical origin of genera and species.

The history of the class is almost coeval with the century. Before that time the Brachiopoda were generally regarded as curiosities; or, if their molluscan character was recognized, they were usually grouped in that class of Lamellibranchiate bivalves which it is once again the fashion to call Pelecypoda. Cuvier was the first to determine correctly the natural affinities of the Brachiopoda (23). He placed them in a class by themselves at the base of his sub-kingdom, Mollusca, and gave them their not over-exact name of Brachiopoda, which Duméril subsequently adopted. Milne-Edwards classed them later with the Polyzoa and Tunicata in a separate sub-kingdom to which he gave the name of Molluscoida, or the mollusc-like animals.

The actual affinities of the Brachiopoda have been the subject of much debate and discussion. There is now a general tendency to revert to the classification of Cuvier. The early worm-like larval stages considered by Steenstrup, Morse (54, 55), and Kowaleveski as proof of the vermian affinities of the Brachiopoda are regarded as "recapitulatory phases" indicative of the parallel descent from one common source of the widely-divergent animals belonging to the sub-kingdoms Annulosa and Mollusca.

[1] These figures refer to Pl. V., which will accompany the conclusion of the article when the Bibliography will be given.—A. C.

I have seen no reason to change the opinion, strongly expressed in 1881, that where the Brachiopoda go the Polyzoa must follow (16). A parallel arrangement of the simpler hingeless forms with the inferior Polyzoa—a very instructive and significant series—reveals so many anatomical points in common that it is difficult to affirm any closer relationship. Milne-Edwards' name may still be retained for this group, from which, however, the Tunicata have been almost unanimously ejected. Conchologists have an inveterate habit of figuring the shells, from the physiologist's point of view, wrong way up.[1] If we turn the *Lingula* right side up we see at once the resemblances to the inferior class of moss-animals.

The possibility of demonstrating descent with modification among the Brachiopoda, so numerous in form and so persistent a race of organisms, early attracted the attention of Darwin. He was much struck with a series of *Spiriferæ* which had been arranged on a tablet to illustrate the succession and variation of species of that genus by Mr. J. W. Salter, Palæontologist of Her Majesty's Geological Survey, in the Jermyn Street Museum.

Since these lines were written a further instalment of Hall and Clarke's great work on the Palæozoic Brachiopoda has been issued (38, 39). By a singular coincidence I find that these authorities therein propose a classification of the numerous species of the genus *Spirifer* into six sections or groups of species, according to their external ornamentations and because it serves to indicate within the integrity of the genus lines of progress leading to resultants which are no longer congeneric. In other words, specific variations among the Spirifers lead up to the evolution of other spire-bearing genera.

It is now more than thirty years ago since Darwin first addressed a letter to Thomas Davidson, historian of the British Fossil Brachiopoda, suggesting that "no one could work out the subject better than he." "I am inclined to suspect," he wrote, in April, 1861, "that on the whole the evidence of the Brachiopoda would be favourable to the notion of descent with modification. Many curious points would occur to anyone thoroughly instructed on the subject who could consider a group of beings under the point of view of descent with modification" (p. 24; 29).

"Mr. Davidson is not at all a full believer in great changes of species, which will make his work all the more valuable," wrote Darwin subsequently, with characteristic fairness, to Robert Chambers, author of "The Vestiges of Creation." The correspondence between Davidson and Darwin on this subject was afterwards published in "The Life and Letters of Charles Darwin," edited by his son, Francis Darwin, and will be found on pp. 366–367 of the second volume of that most interesting record.

In 1876 Davidson published a French edition of the excellent

[1] I am glad to note that Prof. Alpheus Hyatt has reversed all the figures of the illustrations to his remarkable memoir "Phylogeny of an Acquired Characteristic," a powerful argument opposed to the Weismannian hypothesis, issued in the Proceedings of the American Philosophical Society, vol. xxxii. No. 143.

paper entitled, "What is a Brachiopod?" (29), issued in the Geological Magazine two years later: he therein stated, apparently considering the comparatively few species that survive rather than the many that perished—"We have no positive evidence of those modifications which the theory involves, for types appear on the whole to be permanent as long as they continue, and when a genus disappears there is no modification, that I can see, of any of the forms that continue beyond so far as the Brachiopoda appear to be concerned" (29).

Two years later Joachim Barrande, a French Legitimist, and formerly tutor to the Comte de Chambord, whose long exile from France he shared in Bohemia, published an epitome of the general results of his own vast labours in the Silurian system of Bohemia (1). This distinguished palæontologist stated independently that he could adduce no evidence derived from his study of the Silurian Brachiopoda of Bohemia of the existence in that area of the modified descendants of antecedent species (1), He therefore felt unable conscientiously to affirm from personal knowledge that the Brachiopoda would be of much use in demonstrating the truth of the principle of Evolution. Barrande died of grief a few days after the funeral of his king and master, Henri Cinq King of France. But one would hardly expect an adherent of the "white flag," a Legitimist, a believer in the divine right of kings, to be a convert to Evolution. Allowance must always be made for the personal equation and effects of the environment.

Davidson always admitted that varietal changes take place of such a marked character as to make it difficult to define the species, and this led him to express the belief that such groups of species were not of independent origin. He seemed thus to be convinced of the extreme variation of certain species whilst maintaining the permanence of genera.

Now the tendency of all modern research appears to demonstrate the extreme elasticity or flexibility of genera. At times uncertainty prevails as to what constitutes specific, as distinct from generic, characters. We are confronted with the problem of isomorphism, or the apparent *external* similitude of species generically differentiated by marked peculiarities of *internal* structure. So that we often get species, heretofore referred on external characters to one genus, split up into several genera or subgenera, based on divergent characters of internal structure. The fossil *Orthidæ*, for instance. This occurs all along the line, for the recent species of what was formerly termed the genus *Waldheimia*, of King, are now divided into six or seven genera and subgenera; although, judging from external characters alone, it is not always easy to differentiate them, for specific features sometimes change less than generic ones. The reasons for the separation are not fully apparent, until we come to investigate the transitional stages of their individual development, which reveal passing phases we may expect to find, nay often have found, characterizing adult forms fossilized in antecedent strata. Others are predicable—just as some of the earlier stages of the

ancestry of the horse were predicated and have since been actually discovered in a fossil condition.

During the eight years that elapsed between the publication of the English edition of "What is a Brachiopod?" in 1877 (29) and that of his "Summary of the British Fossil Brachiopoda," in 1884 (28), it is surprising that Davidson had not modified his opinion on the matter. We find him stating (p. 386 *et seq.*) that he still found the subject of the descent with modification of the Brachiopoda "beset with so many apparently inexplicable difficulties that year after year has passed away without my being able to trace, in a satisfactory manner, the descent with modification among the Brachiopoda which the Darwinian theory requires." He appears at most to have admitted a kind of "dual control"—species sometimes modified themselves; genera originated in a different manner. But, really, the supervision of generic creation and extinction would have been no sinecure.

"It is probable,"—we now quote *verbatim*,—"that at least a large proportion, if not all of so termed species, may be nothing more than modifications of shapes by descent of a limited number of primordial types; but it is very difficult in the present state of our information to show *passages between the genera among the Brachiopoda*,[1] so well as among other groups of animals, which the theory of Evolution absolutely requires" (28).

In 1894, thanks in a great measure to the life-long researches of James Hall and the sagacious labours and enlightened views of John M. Clarke, Palæontologists to the Geological Survey of New York, it is no longer possible to deny the existence of such passages between many of the Palæozoic genera. Much of the evidence was really accumulated by both Barrande and Davidson, although they failed to recognize its full significance (38, 39, 40, 41).

The Evolution of the Brachiopoda was the subject of frequent friendly discussions between Mr. Davidson and myself, but we never agreed upon it, for being ultimately convinced of the general application of the law of the Evolution of organic life on the earth, a sense of humour prevented me from considering the Brachiopoda as specially created for brachiopodists to describe. My views were sufficiently indicated in an article contributed on the Molluscoida (*Brachiopoda* and *Bryozoa*) (16) to the fifth volume of "Cassell's Natural History," in 1881. But it is one thing to assert, *à priori*, the logical postulate: another to substantiate it by cumulative testimony, as Hall and others have recently done. Truly, as Paley has well said, "they alone discover who prove."

One great stumbling-block in Davidson's way was his strong belief in the immutability of genera. This conviction is fully expressed on p. 391 of his "Summary" before referred to (28). In a footnote he gives a list of 23 Tertiary and Cretaceous genera regarded as valid by foreign authors, and rejects their claims to generic rank. Of these 23, twenty at least are now recognized by

[1] The *italics* are my own.

Beecher, Dall, Douvillée, Deslongchamps (34), Fischer, and the Œhlerts (63), as forming, in many instances, natural connecting links or passage-forms between other genera, and the remaining three were ultimately accepted by Davidson himself in his last posthumous "Monograph on the Recent Brachiopoda" (27).

He had evidently an almost invincible repugnance to the multiplication of generic names and definitions. "Were we to indulge," he wrote in 1884, "the system of making genera out of every trifling and unimportant difference, the number would become so great that it would not only confuse the subject, but render the study of the Brachiopoda one of repulsive difficulty" (28).

The modern school of *fin du siècle* brachiopodists consider that it is the combination, under one generic name, of species with divergent internal features which perpetuates this confusion. They look upon such forms, as for convenience we call genera, as natural occurrences in nature, not as mere arbitrary scientific abstractions designed for the gratification of those nomenclatorial propensities apparently inherent in mankind since Adam modestly named all creation that was known to him. If a genus comprises an assemblage of forms of which certain characteristics are universally predicable, it seems but reasonable to conclude that any deviation from the universal rule should form an exception and be placed in another genus. Up-to-date biologists, therefore, prefer to restrict the number of species, regarding many forms of so-called species merely as divergent resultants from age and environment, and consider the erection of "trifling" differences into generic or subgeneric characters as perfectly justifiable, so long as they are found to be permanent features of adult structure (4, 5, 6).

These minute generic divergences from the original type are justly regarded as so many links in the chain of the generic descent of the race in general, by which they can trace collateral branches with their numerous ramifications. That some of the main lines die out while the collaterals and their respective ramifications continue to be represented, is nothing unusual in genealogical investigations. In other cases the main line persists, as in *Lingula*, whilst many of the smaller branches of the linguloid stock became extinct and left no immediate descendants.

Just at present we are willing to admit that the study of the Brachiopoda presents many difficulties to one occupying—like myself—a neutral vantage ground, respecting the older palæontologists whose valuable labours in many cases alone rendered possible the significant deductions of that New School with whose methods I am in full sympathy. But we must not be deterred by difficulties, however "repulsive" they may be, from the pursuit of truth, and, as students, we must learn to adapt ourselves to the new methods and novel nomenclature, the results of which will in the end clear up the minor questions which still obscure a complete demonstration of the evolution of the Brachiopoda. It is well to remember, as Lowell has well said, that "the foolish and the dead alone never change their opinions."

NOTE.—It is time some examination was made of the well-worn argument with regard to the "unchangeable persistence of the Brachiopoda" once more repeated by that brilliant and popular writer, Prof. Henry Drummond, who is supposed to reduce the hard facts of science to the mental pabulum adapted to modern social innocents (Ascent of Man, 1894). The facts respecting this "unchanged persistence" and the percentage of survivals will be duly presented in a textual and tabulated form. Hence, it will now suffice to state that out of 47 families only seven survive. One genus out of the 24 included in the ancestral order ATREMATA persists, from the Lower Silurian upwards, and that is *Lingula*. But this "persistent" *Lingula* was preceded by a number of antecedent forms many of which died out before its generic characters were established. Three genera out of 31 members of the divergent order NEOTREMATA still survive; only one, *Crania*, "persists" from the Lower Silurian upwards. Both these are relatively shallow water forms, mostly rock-fixed and shore haunters. The sole survivor out of 82 genera of the order PROTREMATA belongs to a secondary (Thecidoid) type. The remaining known genera, about 140 in number, of which somewhat less than thirty survive, all belong to two out of the three main subdivisions of the fourth order of TELOTREMATA, which was scarcely differentiated in the Lower Silurian age. Taking Mr. Schuchert's careful estimate of genera (tabulated March, 1893), we find that, at a rough estimate, two Palæozoic genera out of 175 all told "persist." Thus the rate of "unchanged persistence" among the Brachiopoda is less than one per cent. of the generic mutations evolved—or, say one per cent., making full allowance for generic differences determined by Hall, Clarke, Beecher, and others since that article was issued. Neither of the so-called "persistent genera" appears in the primordial fauna, and the recent species of both these composite genera bear unto this day visible traces of their obolleloid and paterine Cambrian ancestors. So much for the unchanged "persistence" of the Brachiopoda. Radical stocks, as Professor Hyatt has recently shown (*loc. cit.* 44*), may continue to be represented, but not by "persistent" identical generic types. Among the Tetrabranchiate Cephalopoda the result is even more marked than among the Brachiopoda, as the generic term *Nautilus* is strictly applicable only to recent species, and possibly to some Tertiary offshoots of the main *Nautiloid* persistent stem and primordial radical.—A. C.

(*To be concluded in our next Number.*)

IV.—A COMPARISON OF THE PEBBLES IN THE TRIAS OF BUDLEIGH SALTERTON AND OF CANNOCK CHASE.[1]

By Professor T. G. BONNEY, D.Sc., LL.D., F.R.S., etc.

THE pebble-bed in the Trias of Budleigh Salterton has attracted much notice, but I am not aware that its materials have been compared with those of the corresponding bed in the Bunter group

[1] A paper, read in substance before Section C, at the Meeting of the British Association at Oxford.

of the English Midlands.[1] For this purpose I recently spent a short time at the former place, and think the results worth placing on record. I select for comparison the pebble-bed on the more northern part of the wild Staffordshire moorland called Cannock Chase, with which I happen to be especially familiar. The points of resemblance shall be noted first, those of difference afterwards.

The outcrop of the Devonshire pebble-bed is usually a hilly moorland, naturally overgrown with bracken, gorse, heath, and ling. There are scattered copses of Scotch firs (planted). That is the characteristic scenery of Cannock Chase. The pebbles are numerous, and lie in a sandy matrix. They vary from subrotund to well rounded, the larger specimens being sometimes almost subangular. They seldom exceed a foot in the longest diameter, are very commonly from 3 to 5 inches, and smaller specimens are abundant. Here and there are intermittent beds of sand, nearly or quite free from pebbles, in thickness from a few inches to two or three feet; false-bedding is often distinct; the pebbles are occasionally impressed one by another, though rather less frequently than in Staffordshire, for they are not, I think, quite so closely packed. But the general resemblance of the two deposits is very marked.

Where I had the best view of the relation of the Budleigh Salterton pebble-bed to the underlying and overlying deposits, I found it pass rather quickly, and apparently regularly, into the marl below, the pebbles, perhaps, generally being a little smaller than usual for the last few inches, and the matrix more marly. The pebbles also run rather small at the top, for any larger than a duck's egg are rare in the last four feet; the upper surface of the bed is usually even, and it is succeeded by a thick deposit of false-bedded red sand, which has a general resemblance to the Bunter sand of the Midlands.

Next as to the materials of these pebble-beds. Vein-quartz is common in both. Quartzites of more than one variety are yet more common. Speaking in general terms, the same types occur in both deposits, though not in the same proportion. I reserve, however, further particulars till I come to mention the differences, merely observing that in both a fairly hard quartzite, the surface of which has a somewhat speckled look, due to the presence of very small grains of felspar, is rather common. But I was surprised to find at Budleigh Salterton specimens of the moderately coarse hard quartz-felspar grit, which so closely resembles the Torridonian of Scotland, to the presence of which in the Midlands I have already called attention. Specimens of a dark-green to almost black rock occur in both deposits, which will presently receive a fuller notice. Both also contain some felstones, with more basic rocks of compact structure and purplish colour; also granitoid rocks in a very rotten condition. The last are rare in the Midlands; still rarer and more obscure at Budleigh Salterton. In neither region are fossiliferous

[1] A very full description of the pebble-bed, with notes on the pebbles by Mr. Carter, and references to literature up to 1880, is given by the late Mr. T. Davidson. "Palæont. Soc." vol. xxxv. (1881).

quartzites or grits at all common; indeed, if I may reason from my own experience, I should say there was about as good a chance of finding them in the one place as in the other.

Next as to the points of difference. On the whole, I think, pebbles of about four inches diameter and from that to a foot are rather commoner at Budleigh Salterton than on Cannock Chase. Certainly in the pits on the latter one sees a smaller number of the subrotund to subangular stones from 8 inches to a foot long. There is also a rather marked difference in the form of the better-rounded specimens. Pebbles which are nearly prolate spheroids occur in both deposits, but that is the common shape in Staffordshire, while those in Devonshire are more usually oblate. This is very conspicuous in walking by the seaside along the pebbly beach, probably because the specimens have been finished off, possibly even to some extent selected, by recent wave-action. The eye is at once arrested by a certain flatness in outline, which may be indicated by saying that the dominant type of pebble is an ellipsoid, the diameters of which are roughly as the numbers 5, 4, and 2.

As to materials, vein-quartz pebbles are, I think, rather commoner in Devonshire. Of the quartzites, both districts show several varieties, varying from merely hard grits to rocks so uniform in structure and so completely cemented as to break with an almost smooth surface and subconchoidal fracture. The latter type, however, is not common at Budleigh Salterton and is abundant on Cannock Chase. Nevertheless, I found one or two specimens at the former place which cannot be distinguished from the noted liver-coloured quartzite of the latter. Slightly felspathic quartzites are commoner at Budleigh Salterton, in fact a minute "speckling" of the stone is one of the first things which catches the eye.[1] The dark compact rocks are rather common at Budleigh Salterton, not so on Cannock Chase. In the former place they are very frequently almost subangular and as large as the rest, pieces full 6 inches in diameter not being rare; on the latter, they are generally rather small and better rounded. Fragments chipped from five of the dark-green pebbles were powdered and examined under the microscope.[2] All showed tourmaline, so two of them were sliced. Both consisted mainly of granular quartz and tourmaline; the latter mineral occurring mostly in grains, sometimes almost films, occasionally in prisms and needles. The first are generally brown in colour, now and then passing into a dull blue; their shape and aspect often suggest that the mineral originally was a ferro-magnesian mica, and that the replacements may not always be complete. The prisms vary from brown to blue, and the needles appear to represent a paler variety than the others. Both these forms are most abundant in small quartzose veins which traverse the pebbles. Here sometimes the sides and one end of a crystal are well developed. The general aspect of the specimens suggests

[1] Here also the lighter coloured stones become a whiter colour, after they have been cleaned from the matrix, than they do in Staffordshire, though perhaps this difference may not be of much value.

[2] I am indebted to Miss C. A. Raisin, B.Sc., for kind help in this matter.

a sedimentary rock which has been affected by contact-metamorphism and has had its aluminous constituent converted into tourmaline.

Another pebble, dark in colour, with thin bands or streaks of a grayer tint, was sliced after tourmaline had been detected in the powder. It consists mainly of quartz and the above-mentioned mineral, but indubitable tourmaline is present in a quartzose vein. In the paler bands quartz predominates; in the darker mica or tourmaline. In the latter are some small rounded spots of chalcedonic quartz. These suggest the possibility of an organic origin (? radiolaria), but I cannot venture to speak more positively. The rock, I believe, was originally a stratified clay and silt, which subsequently was affected by contact-metamorphism, followed by the production of tourmaline.

Lastly a pebble (from the beach) which exhibited well-marked bands of a gray and a dull-brown colour, very minutely granular, was sliced, after its powder had suggested the presence of tourmaline. The lighter layers are mainly chalcedonic quartz with small films of brown mica or tourmaline, the darker mostly of the last mineral, which also occurs in grains and prisms, these being perhaps more common than in the other specimens. Here also are veins with prismatic and acicular tourmalines, but in the darker layers spots or lacunæ occur which are occupied by quartz and tourmaline, sometimes prismatic, often acicular, the latter seemingly paler in colour. The slice also contains some granules of a darkish mineral, occasionally opaque, but sometimes rather translucent, and of a muddy golden-brown colour, possibly cassiterite. This rock appears to have had the same history as the last one.

These specimens correspond with some of the fine-grained tourmaline-bearing rocks which may be obtained from the zones of contact-metamorphism around the granite masses of West Devon and Cornwall. I did not, however, observe any of the coarse-grained quartz-tourmaline rocks, which I have often seen in the latter county; for instance, near St. Austell and to the west of Penzance. Dark green pebbles with a general resemblance to certain of the above-named occur, as already stated, in the Midlands; but these, so far as I have examined them microscopically, though they contain tourmaline, present considerable structural differences, and are not suggestive of contact-metamorphism.

Fragments of igneous rocks, while not abundant at either locality, are distinctly commoner and more varied in Staffordshire; there, with the same amount of work, I could collect at least a dozen varieties of quartz-felsite; in Devonshire I found at most two or three, only one of which was anything but rare. The same is true of the more decomposed rocks, both basic and crystalline. In Staffordshire I should have seen ten specimens of igneous rocks where in Devonshire I saw one. In the former county also, what may be called "miscellaneous sedimentaries," such as sandstones, mudstones, chert from the Carboniferous Limestone, etc., are commoner.

I was not able to recognize any of the following rocks: the gneissoid and hornblendic rocks, the serpentines or gabbros of the

Lizard district, the schists of the Start, the granites of Cornwall or Dartmoor, the Devonian limestone, the Palæozoic slates (perhaps hardly to be expected) or their associated grits of the south-west region. Among the quartzites and quartzitic rocks a few might be Devonian, and some others probably represent the quartzites (earlier Palæozoic) of southern Cornwall,[1] but the majority did not quite correspond with any of the rocks which I have seen *in situ* in the South-west of England, and, though my knowledge is nothing like complete, I have sampled a fair number. Moreover, I was not reminded at Budleigh Salterton of the fragments in the Meneage conglomerate. The felstones may represent some of the western elvans, but, as already said, they are comparatively rare; the other igneous rocks, still more rare, are not characteristic.

The materials, then, seem not generally to represent the rocks now exposed in Cornwall and Devon. I had expected to find a large proportion of rocks from this region, as in the case of the breccias, which are so fully exposed further west. I was surprised to observe a very marked difference in this respect, and a much closer correspondence with the lithological character of the Midland pebble-beds. The fragments are far better rolled than those of the breccia beds, and thus very probably have had a much longer journey. I do not, however, suppose that they are derived from the same source as the pebbles in the Midlands. The ascertained facts as to the distribution of the Trias, which it is needless to recapitulate, make this practically impossible, and the fairly constant direction of the false bedding indicates the action of currents from a more or less western quarter. But the presence of the curious quartz-felspar grits, and of quartzites resembling those of North-west Scotland, including even the peculiar liver-coloured type, suggests the possibility that the ancient mass of Archæan crystallines, which once swept round from the Scoto-Scandinavian region to North-western France, may have been fringed in more than one district with rocks of Torridonian and Durnessian types.[2]

V.—Questions and Answers on Ice-Motion.

By Rev. E. Hill, F.G.S.; late Tutor of St. John's College, Cambridge.

A PASSAGE in Mr. Deeley's letter in the December Number suggests to me that some readers may like an opportunity of clearing their thoughts on some points connected with the motion of ice. After a tutor's fashion, I will ask some questions. Let any reader consider the answers he would give, and then compare with mine which shall follow.

If ice be a viscous body, how can the Antarctic ice stand "presenting a vertical wall to the ocean, 150 feet high"? So, in a better known case, how can the walls of crevasses in a glacier retain their

[1] Mr. Pengelly (Geol. Mag. 1878, p. 238) considered the fossiliferous quartzites to be practically identical with those of Gorran Haven.

[2] See a very suggestive paper by Mr. Ussher, Quart. Journ. Geol. Soc. vol. xxxv. (1879), p. 245.

shape? And again, how can the so-called Glacier-tables subsist? Why does not the ice of the stem squeeze out under the weight of the top stone, and let it down?

I do not write for mathematicians and physicists, who will smile at these questions, and may improve my solutions. I write for all who having read them must hesitate before answering. These would be my answers if asked the questions in an examination. Reversing their order: A *Glacier-table* is due to a melting away of the exposed surface by the sun, while the ice beneath a large stone is unmelted. If the surface sinks by that waste faster than the protected stem shortens by compression, the stem will lengthen. The squeeze *might* make the stem thicken, and *should*, unless the waste of thickness by evaporation, etc., exceed the increase which the compression can produce. *Crevasses* are due to a tension acting on the mass of ice. If viscosity cannot close the fissure faster than tension is opening it the crevasse once open must remain so. When the flow of the glacier has carried the ice past the tension-position the crevasses will close. The *Antarctic ice*, if a viscous body, must flow outwards into the ocean until it is ready to float, and till its strains from floatation or other causes are more than its strength can bear. At this point masses will break off and float away, leaving where they broke from the wall-like face described. A similar wall on a small scale due to a like cause bounds (or did, when I saw it, bound) the Aletsch glacier along the Marjelen See.

As another question: If that ice be viscous, why does it not flow up the Marjelen side-valley as well as down the main Aletsch channel? Answer: It does so flow, to an extent measured by the amounts melted away from that face, and broken away.

The following, less simple, problem may enable some readers to test their ideas on the matter of ice moving "uphill." I suppose everyone has seen the undulated erection called a switch-back railway. Suppose a continuous train of cars in contact along the whole length of rails: if free to move, the weight of the topmost ones will force the whole train forward. Suppose the cars replaced by blocks of ice, separate but in contact; no doubt a similar motion will ensue. But now, suppose the interstices between successive blocks all filled up, so that they are united into one continuous thick ribbon, or undulated beam of ice; what will happen then?

Then, in motion forwards, convex parts would have to compress, concave parts to extend. I have no doubt that if the upper end be at a level high enough, some motion will result. What that motion will be, whether the same forward flow as of the separate blocks, or a shattering to pieces of the ice at points of greatest stress, or a thickening in the hollows, or a combination of these, depends on the amount of breaking-strain and the rate of viscous flow. If, however, the stress be enough to cause any viscous yielding, the ice in the hollows will thicken, the mass forced uphill will increase, the force required to move it will in turn increase, and the thickening be accelerated (assuming, of course, a continual supply of ice at the starting-point); whilst, on the other hand, it may be seen that con-

versely the ice on the summits would have a tendency to thin. If the erection have parapets of small height, when the ice-stream thickening rises higher than the parapets, parts will ultimately break away. This answers to the Aletsch glacier flowing past the opening of the Marjelen See valley. But if the parapets were very high, I think that ultimately the hollows must fill up to at least the levels of the summits, and so produce an undulating but generally descending slope. Such, indeed, we see in actual glaciers, if there be rock-basins hidden in their beds.

I have not sought the figures for the viscosity and breaking-strain of ice. Blocks in a frost can lie about without perceptible change; the roof of the Esquimaux's snow-hut does not sink down on his head during the night; the walls of the Montreal winter-castle remained as long as they were wanted. Perhaps ice does not begin a viscous flow till the force reaches a sufficient amount. A stick of sealing-wax may lie on its side for months without noticeable alteration, but "hang a weight to a bar of it, and it will yield" (Lord Kelvin, "Popular Addresses," vol. ii. p. 344).

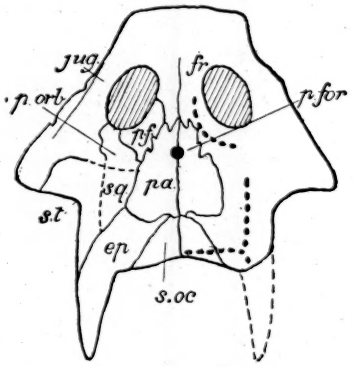
Practically we see that the Swiss and Greenland glaciers do move as viscous bodies. They follow paths of least resistance, and these paths are much the same as water would follow. I have been asked—What should happen when a glacier from a side valley debouches on to the top of a larger one in a main valley? Much the same as what happens when a tributary flows into a main river, as may be seen where the glacier from under the Sparren-horn debouches on to the Aletsch. Water is viscous and ice is viscous, though in very different degrees. "Water does not run uphill," we say, but in a pipe it does. Ice can move uphill if it must, but we do not find it flowing uphill if there be an equally eligible down-hill path available. This is the argument of a letter of Professor Bonney's in Nature (22nd February, 1894), which met with little reply. The subject tempts me to a further pursuit, but I will not transgress the limits which I indicated in beginning.

VI.—Note on a Specimen of *Keraterpetum Galvani*, Huxley, from Staffordshire.

By C. W. Andrews, B.A., B.Sc., F.G.S.
of the British Museum (Natural History).

In a collection of Coal-measure Vertebrata made by Mr. J. Ward, of Longton, and recently acquired by the British Museum, there is an imperfect, crushed skeleton of a small Labyrinthodont from the Ash-coal Shale of Longton Hall Colliery, Staffordshire. This specimen was noticed by Miall in 1874 in his British Association Report on the Classification of the Labyrinthodonts, where it is referred to *Urocordylus*, some of the characters given in his diagnosis of that genus being taken from it. Some measurements of the skull were added. The same specimen was afterwards figured in Mr. Ward's paper "On the Geological Features of the North Staffordshire Coal-field" (Trans. N. Staffs. Instit. Mining

Engineers, vol. x. 1890, pl. ix. fig. 2) as *Keraterpetum Galvani*. This figure, which is natural size, unfortunately does not show any details of the structure of the skull, some account of which may be of interest.



Dorsal surface of skull of *Keraterpetum Galvani*, Huxley. *s.oc.* supra-occipital, *ep.* epiotic, *sq.* squamosal, *s.t.* supra-temporal, *pa.* parietal, *p.f.* post-frontal, *p.orb.* post-orbital, *jug.* jugal, *fr.* frontal, *p.for.* parietal foramen. The thick dotted lines on the right side of the figure mark the position of the lateral line pits. Twice natural size.

The general outline of the skull (see figure) closely resembles that shown in the figure of the type specimen of *Keraterpetum Galvani* given by Huxley (Proc. Roy. Irish Acad. vol. xxiv. 1867, pl. xix. fig. 1). The snout is blunt and rounded, and the hinder border of the skull is produced backward into two pairs of cornua, of which the external pair, formed by the quadrate region, is much less prominent than the internal pair (epiotic), and as far as can be seen, does not extend quite so far backward as the base of the latter. The exact limits of the outer pair are rather indistinct, and of the inner pair, that on the animal's right is broken away. In the posterior region of the skull, the dorsal side of which is exposed, the outer surface of the cranial bones is well seen, but in front only the impressions of bones remain and it is almost impossible to determine their arrangement. On the counterpart, however, this can be made out to some extent. The external surface of the bones is covered by a sculpture of pits arranged roughly in parallel or radiating lines, an ornament closely resembling, on a small scale, that seen in *Loxomma*. The pits are close together, being separated only by narrow ridges. At the bottom of many of them there is a small pore with a slightly raised rim, such as occur also in *Loxomma*. These pores were probably connected with dermal glands. Running directly forward from the hinder border of the skull, near the inner side of the base of the epiotic cornua, there is on either side a row of larger pits which can be traced to the neighbourhood of the eye, where it is probably continuous with a still more distinct series forming a curved line parallel to the

postero-internal border of the orbit. Across the occipital region there is a transverse row of large pits connecting the two lateral series. The whole mark the position of the lateral line system of sense organs.

The sutures between the cranial bones are for the most part obscure, but by careful examination with the lens the arrangement, shown in the figure, can be made out with a fair degree of certainty. The supra-occipitals together form a roughly triangular area, of which the base is the hinder border of the skull; the anterior angle is truncated by the parietals, while the sides are united by suture with the epiotics. These latter, which bear the long, backwardly projecting cornua, are bounded by the parietals in front, and the squamosal antero-externally. The squamosal abuts on the parietal internally, but its outer boundary is not clear. I believe that traces of a suture running antero-posteriorly close to the pits of the lateral line, can be made out. If this be so the bone external to this suture, and forming the angle between the outer and inner cornua, will be the supra-temporal. It is separated externally by an obscure suture from a bone which may be quadrato-jugal. The parietals together form a triangular area with rounded posterior angles. In the median suture about two-thirds of its length from its hinder extremity is the round pineal foramen. The outlines of the post-orbital and post-frontal are not very clear, but the lines given in the figure mark their approximate boundaries as shown in the counterpart of the specimen. On the right side a portion of the jugal can be seen forming the outer border of the orbit, but it is crushed and obscure, as also are the maxillæ, premaxillæ, nasals, etc.

In the type specimen of *K. Galvani* the actual sutures between the cranial bones are not shown, but judging from the figure given in the memoir above quoted, their form and arrangement must have been the same as in the present specimen, which was rightly referred to that species by Ward.

If the skull here described be compared with that of *K. crassum*, figured by Fritsch (Fauna der Gaskohle, Bd. 1) we find that very great differences exist between the two species. The most notable of these are: (1) In *K. crassum*, if the epiotic cornua be left out of account, the hinder border of the skull forms a straight line, while in *K. Galvani*, the occipital border is far behind the outer (quadrate) angle (see figure). (2) In *K. crassum*, Fritsch figures the epiotic cornua as distinct elements articulating by a kind of ball and socket joint with the epiotic (?) bones.[1] In *K. Galvani* the cornua are simple backward prolongations of the epiotics.

(3) In the former the orbits are separated by a space equal to

[1] The morphological value of these separate epiotic cornua is doubtful. Fritsch himself suggests that they may either be simply portions of the epiotic bones or may constitute the whole of them, the bone to which they articulate being in that case undetermined. It may be pointed out that these elements occupy the position of the post-temporals of fishes, and may possibly indicate the former occurrence among the *Stegocephali* of a connection between the posterior region of the cranium and the pectoral girdle.

about six times the diameter of one of them, while in the latter the interval is only equal in width to one of the orbits. (4) The roofing bones of the skull, at least in the posterior region, differ considerably in form and arrangement in the two species. This is particularly noticeable in the case of the parietals, which in *K. crassum* form together a rectangle about twice as long from side to side as from before backwards. (5) In *K. crassum* the sculpture of the cranial bones consists merely of a few widely separated pits.

The differences between these two forms appear, therefore, to be so great as to render it impossible to refer them both to the same genus, and *K. crassum* should, therefore, be referred to as *Scincosaurus crassus*, the name originally applied to it by Fritsch in 1875.[1]

Concerning the resemblance of this skull to that of *Urocordylus* it is difficult to speak with certainty, since the skull of the specimen on which Huxley founded that genus is unfortunately very badly preserved (see *loc. cit.* pl. xx. fig. 1). In it no epiotic cornua are to be seen, and these structures are also wanting in *U. scalaris*, Fritsch, the skull of which also differs widely in other respects from that of *K. Galvani*. In *U. reticulatus*, Hancock and Atthey,[2] these cornua are well developed, but the specimen resembles *K. Galvani* in so many respects that it is not improbably a young individual of that species.

The remainder of the skeleton is not well preserved. The serration of the upper border of the neural spine, characteristic of the family Nectridea, is clearly visible in some of the dorsal vertebræ. The whole of the tail is missing, so that comparison in that respect with *Urocordylus* is out of the question. A large part of one of the lateral plates of the thoracic buckler is preserved, and in form exactly resembles the bone figured as *scapula* (?) by Huxley (*loc. cit.* pl. i. fig. 1). In this specimen the surface of the bone shows a sculpture of pits in rows radiating from its posterior spine-like prolongation. Scattered about the skeleton there are several scutes with a sculpture like that of the cranial bones, of which, indeed, some may be fragments; the others are probably the larger scutes figured by Huxley, pl. xix. fig. 3. Of the limbs only obscure traces of the femur and humerus remain.

NOTICES OF MEMOIRS.

Abstract of the Fourteenth Report to the British Association on the Earthquake and Volcanic Phenomena of Japan.

(Drawn up by the Secretary, Prof. J. Milne, F.R.S., F.G.S.)

THE first part of the Report gives a list of the earthquakes recorded in Tokio during the last year. The second portion gives an account of observations made with horizontal pendulums. These pendulums consist of a horizontal boom about five feet in length held up by a fine brass wire. At the extremity of the boom

[1] Sitzungb. der k. bohm. Ges. der Wissensch. 19 März, 1875.

[2] Nat. Hist. Trans. Northumberland and Durham, vol. iii. p. 310.

there is a light metal plate with a slit in it parallel to the length of the boom. Underneath this floating slit, but at right angles to it, there is a narrow slit in the top of a box. Light passing through the two slits goes into the box as a point, which is received on a drum carrying a photographic film. These instruments, which are usually arranged in pairs, are placed to point N.W. and N.E. or parallel and at right angles to the strike of a distant range of hill. Two have been used at Kamakura on the solid rock, and two at Yokohama and one at Kanagawa on the soft tuff rock. These instruments were in caves. Also I have had two underground on the alluvium and one at my house on a solid stone column in Tokio.

The movements recorded have been as follows:—

1. *The Wandering of Pendulums.*—All the horizontal pendulums, wherever situated, have slowly wandered from their normal position. Those situated on the rock have often gradually moved to one side and then returned, the double excursion usually taking from two days to a week. These wanderings might be due to a local warping of the supporting column; but inasmuch as it has generally happened that the periods of great movement and of comparative rest have coincided in time, it would seem that the movements are in all probability due to a more general cause. Because certain movements have usually been marked (but by no means always) at or after a rainfall, it seems possible that they may be connected with fluctuations in the volume and flow of underground water,—the pendulums nearest to this water moving the most.

2. *Daily Waves.*—In no instance have I observed a diurnal movement of the pendulums at stations situated on the rock. In Tokio the movements occur underground and on the surface; they happen at the same time and they are proportional in magnitude. At my house, for example, at about 10 a.m. a tilting commences on the N.E. side, and it reaches a maximum of one or two seconds of arc about midnight, when a sinking takes place until about 6 a.m., when the pendulums remain fairly steady for some hours, after which they again commence to rise. Because it was observed that the movements underground were greater than those recorded on the surface, it seemed possible that they might find an explanation in the fact that the underground instruments were nearer to pervious strata in which water fluctuated than the instruments installed in my house. By quickly emptying a well in my garden of about two tons of water which was run off down a hill, the pendulum on a column (104 feet distant) behaved as if the ground had been relieved of a load and therefore had risen on the well side. Because the direction of movement of the pendulums underground and on the surface is away from the side from which during the day the greatest load is being removed by evaporation, it is not unlikely that the main features of the diurnal wave may be due to this cause.

3. *Earth Pulsations* (*Tremors*).—In Italy I understand that earth tremors are as marked underground on the rock as they are on the surface. In Japan, on three underground rock foundations, I have not observed a single case of earth tremors. Both underground and

on the surface in Tokio they are marked, lasting many hours. As I have previously spent so much time in analysing tremor records, the present records remain untouched. Tremors occur with a low barometer, but more generally when there is a steep barometric gradient. It seems possible that these conditions may result in giving the surface of the ground an ocean swell-like motion through the agency of subterranean water.

4. *Earthquakes.*—At Kamakura, on the hard rock, the greatest earthquake motion has been given by the pendulum which records tilting parallel to the dip, suggesting the idea that in this direction there is an easier yielding (like the opening and shutting of a concertina) than there is in a direction parallel to the strike. On March 22, I and my colleague, Mr. C. D. West, watched an earthquake for 1 h. 47 m., during which time the pendulum did not *swing*, but was *forced* backwards and forwards intermittently and with extreme irregularity. These earthquakes are in the form of earth waves and usually come from a great distance. A sharp shock which may be felt throughout Tokio, and at many places in the country, does not disturb the pendulums, and it is difficult to find a blurr on the photographic trace.

REVIEWS.

I.—Geographical Morphology.

Morphologie der Erdoberfläche. By Dr. Albert Penck, Professor of Geography in the University of Vienna.
Ratzel's Bibliothek Geographischer Handbücher. J. Engelmann. Stuttgart, 1894. 8vo. 2 Vols. Vol. I. pp. xiv.+471, 29 Figures. Vol. II. pp. x.+696, 38 Figures.

As the study of geography must have commenced unconsciously in the very earliest days of man's existence on the earth, it is not surprising that this science long ago acquired a rigid conservatism, and lack of scientific method, and a burden of imperfectly defined popular terms. In England descriptive topographical geography is still hampered by this inheritance. Abroad, however, more scientific methods have been rapidly gaining ground, especially in Germany and Austria, and in the United States. In the two first geography reached its highest level in Richthofen's admirable "Führer für Forschungsreisende" and Suess's "Antlitz der Erde." In America the writings of Gilbert and W. M. Davis have founded a school which has advanced the subject with great energy. The works of these authors are, however, bulky and scattered, and a systematic text-book of orography has been a desideratum for some time past. Dr. Ratzel, the editor of the well-known "Bibliothek Geographischer Handbücher," persuaded Prof. Penck to undertake the preparation of one. The task has, however, taken more than ten years to fulfil, for the literature of geodesy, geography, and geology have had to be carefully worked through.

The book consists of three parts. The first deals with general morphology. This will probably be of most interest to geologists, for the subject is barely noticed in geological text-books, and there

is no English geographical manual which gives it adequate notice. In this section much useful information is given in a concise and simple form, upon various branches of geodesy, which can elsewhere be only found in such works as Günther's Geophysik (1885), or Handbuch der mathematischen Geographie (1890). Penck gives the latest conclusions upon the earth's form, and the methods and results of morphometry. Thus in regard to the former, he states that the question of the amount to which the form of the earth deviates from a true ellipsoid of revolution is now reduced to from ± 200 to ± 250 metres. As an example of the latter may be noticed the care with which the altitude systems adopted by different countries in Europe are tabulated and compared; some of these are based on a supposed natural line, such as the British "sea-level," and others on a recognizedly artificial standard, such as the German "Normal Null." No two nations adopt the same standard: thus the levels in Belgium are all 3·39 m. lower than in Germany, and the standard in Ireland is more than 7 feet lower than that of Great Britain. The uncertainties both in the accepted longitudes and altitudes are clearly pointed out. Thus we are warned that even when the latter are based on trigonometrical observations they are not absolute; for the error of refraction cannot be altogether eliminated. The considerable amount of this is shown by two cases quoted: thus Hartl found that the difference of altitude of two points 28 kilometres apart, varied from 13·6 metres at 5 a.m. to 36·2 m. at 10 a.m. And Bauernfeind found that the difference between the summit of the Dobra and the Kapellenberge in Bavaria increased from 25 m. at 3 a.m. to 35·6 m. at 11 a.m.

This part of the book discusses the methods and formulæ used in the determination of the principal measurements of the earth's surface, and also gives the results: thus there are tables of the latest estimates of the ratio of land to water, the area and cubic contents of the different continents and oceans, the lengths of coast-line, the proportions of the continents at given distances from the sea, etc. Two of the most interesting chapters are those on the geographical homologies, which have been recently so graphically illustrated by Prof. Lapworth, and that on the question of the permanence of oceans and continents, of which discussion a useful bibliography is given. In regard to the latter it is interesting to note that the author fully maintains that the specific gravity of the earth below the oceans is greater than below the continents.

In the second part of the work the surface of the earth is described. It is regarded as made up of a number of "land-forms," which are grouped into eight classes: these are plains; heaped up masses, such as dunes and sinters; valleys; highlands (thallandschaft) or the masses cut up by valleys, including plateaux, and some types of mountains; basins or "wannen"; mountains; areas of subsidence; and finally caves and ravines. The agents that produce and mould these various forms are also described. This part of the work is of value as a digest of the classifications and literature of structural geography. It uses throughout the terminology of Richthofen and

Suess, and gives many diagrams to explain that of the latter. The thorough acquaintance with geographical and geological literature, which Prof. Penck's writings always show, enables him to treat this part of the subject most exhaustively and completely. It is, moreover, surprisingly well up to date: for example, Bertrand's recent paper contending for the persistence of lines of dislocation is noticed (and adversely criticised), and Heim's recent description of Pleistocene earth movements in the Alps is included, though these and others were issued only in 1894. The summaries of literature and the collections of local geographical terms are extremely useful. In most things the author is very advanced, but in glacial matters he is strongly conservative: he attributes to ice great erosive power, and thinks that both cirques and rock-basins have been formed by it, and maintains the existence and action of ground-moraines. The two most valuable sections in this part of the book are those describing the distribution, origin, and classification of basins and mountains. It is in reference to these that the works of Suess and Richthofen have had the greatest influence, and where an authoritative statement of the present position of the subject is of most service. The origin of many of the greatest basins is not yet fully settled: thus, in reference to the Caspian, Sjögren's recent explanation of this as a synclinal fold disputes Suess's view that it is a "Senkungsfeld" or area of subsidence.

The last part of the book is entitled "The Sea." It describes the action of the sea by coast erosion, tides, and currents, and by floating ice; it also describes the formation of deltas, mangrove swamps, and coral reefs; it classifies coast-lines, and shows how these have varied both by the rising or falling of the land and by variation in the level of the surface of the sea. Finally comes a description of the sea-floor and a classification of islands. An admirable index of authors and subjects renders the vast number of references to literature in the volumes readily accessible.

The work is not one which can be easily summarized, for it is encyclopædic in its detailed summary of facts and classification. It is a book of reference which is indispensable in every geographical and geological library. J. W. G.

II.—Climbing and Explorations in the Karakoram-Himalayas. By W. M. Conway. Supplementary volume containing Scientific Reports by Prof. Bonney, Dr. Butler, W. M. Conway, W. L. H. Duckworth, Lt.-Col. Durand, W. B. Hemsley, W. F. Kirby, Miss Raisin, and Prof. Roy. 8vo. pp. 127, with Frontispiece Portrait of the Author, and 2 Coloured Maps. (London: Fisher Unwin, 1894.)

THIS Supplement to Mr. Conway's Exploration in the Himalayas, contains the scientific results of his expedition. The first of these is an account of the Eastern Hindu Kush, by Lt.-Col. Durand, who gives an interesting account of the people of this country, especially of Chitral, or "the land of mirth and murder," as he

appropriately terms it from the customs of these people; an instance of whose sanguinary practices we have recently seen recorded in the daily Press. Following this we find a list of measured altitudes taken by means of a barometer up to 22,600 feet, the two higher ones being measured by a Theodolite; that of K. 2, Mr. Conway makes to be 27,750, or 500 feet lower than the measurements given in the Great Trigonometrical Survey of India. These, and the two very excellent coloured maps with their description, are by Mr. Conway, and they practically represent an unexplored country, as previous to 1892 this region had only been roughly sketched out by Colonel Godwin-Austen, who, in 1860–61, examined the lower parts of the glaciers, and marked the position of the chief ridges; it is only since the introduction of mountain-climbing that the details of these large glacial areas have been brought to light, and these we owe to Mr. Conway's indefatigable energy.

The author's work was not solely confined to the production of maps, for he also collected numerous rock-specimens and minerals, plants, and insects: these have been identified by various specialists, and lists are printed giving the names of the specimens, the localities and the heights where they were obtained. They form valuable records, and constitute an excellent example for future explorers.

The rocks, etc., have been examined by Professor Bonney and Miss Raisin, who give a detailed account of the various specimens, which consist of serpentines, diorites, and schists, several of which are said to resemble known Alpine rocks, granites, gneiss, and felstones; one schist in particular is found to contain a new form of hydrous biotite, having an extra large percentage of lime. The sedimentary rocks consist of limestones, slates of possibly Carboniferous age, sandstones, grits, and conglomerates, bearing some resemblance to the older Cambrian rocks of Wales. Among the minerals is a curious jade-like rock, which appears from its structure and analysis to be new, and the authors regard it as an unusual mixture of certain known minerals.

The concluding part is by Professor C. S. Roy, and contains an interesting account of mountain sickness, and a series of pulse-curves taken by Mr. Conway at different elevations, and under very varying conditions, which are all carefully recorded. Although Professor Roy is still unable to come to any definite conclusion as to the real cause of the pulse variations, and whether heart-failure is, or is not, essential in mountain sickness, still these notes will be very useful to any intending mountain-climbers.

III.—An Introduction to Modern Geology. By R. D. Roberts, M.A., D.Sc. 8vo. pp. vii. and 270, with 9 Plates, 51 Figures in the text, and 3 Tables. (London: John Murray.)

THIS little book forms one of the University Extension Series of Manuals intended primarily for the use of Students attending the lectures connected with that movement. It will, however, have a much wider circulation, as it is admirably suited for the needs of any student commencing the study of geology, and the presence of references makes it serviceable to those who are more advanced.

It is a great pity that the author found it necessary to change the title, as the adjective in the present one is very aggressive.

The plan adopted in treating first from the physical, then from the structural, and finally from the evolutionary standpoint of the Earth's crust, is an admirable one, and has been well carried out.

The author very wisely confines himself as far as possible to the geology of the British Isles; this gives the book a special value to beginners as an educational work, for by expounding to students the features of their own country, and explaining the nature of the changes which have gone on, or are still in progress around them, some of which they may be able to verify for themselves, they gain a sounder knowledge of the subject than they would from the description of the more striking changes, etc., which might be collected from abroad, and with which they could only deal on paper.

Still, considering the small size of the book, it is a little extravagant to devote a whole chapter to the volcanic phenomena of the Island of Eigg, but, on the whole, the problems visible there, and the conclusions deduced from them, form an excellent lesson to the student of what may be done in reconstructive geology.

The matter in the chapter on deep-sea deposits might well have been expanded at the expense of three of the illustrations, for we find two whole pages devoted to two views of Brooke's deep-sea sounding apparatus, and one to a beautiful figure of a *Globigerina*, the latter being the only organism figured from these deposits, the characters of the remaining organisms being left wholly to the imagination of the reader; in several cases the bare name only being given, without any reference to its nature or to what group the organism belongs.

The description of the hard parts of a coral as an internal skeleton is hardly worthy of a "modern" book, considering that it has been known for about ten years to be a secretion of the ectoderm and therefore a morphological exoskeleton. Also the definition of the animal of *Globigerina* as a speck of protoplasm which has no parts or organs of any kind is hardly in keeping with the otherwise up-to-date nature of the book.

The illustrations, though in some instances a little lavish, are very good, and many quite new, the coloured maps being especially fine.

REPORTS AND PROCEEDINGS.

Geological Society of London.

December 19th, 1894.—Dr. Henry Woodward, F.R.S., President, in the Chair. The following communications were read:—

1. "The Lower Greensand above the Atherfield Clay of East Surrey." By Thomas Leighton, Esq., F.G.S.

This paper embodies the results of the author's examination of the Lower Greensand of East Surrey during the three years 1892–94; and it is stated that two papers published by the Geologists' Association (vol. xiii. pp. 4 and 163) are to be taken as introductory to

this one. The area discussed in this paper extends from Leith Hill in the west to Tilburstow Hill in the east; and the divisions of the Lower Greensand chiefly referred to, are those hitherto known as the Bargate, Sandgate, and Hythe Beds. The author states that the Lower Greensand of East Surrey shows that formation to consist of beds deposited in a marine estuary or narrow sea, not far from land and within the influence of strong currents, extending generally from N.W. to S.E., so that, without palæontological evidence, no correlation of beds here with those exposed at Sandgate and at Hythe is possible. He arrives at this conclusion by following the outcrop of the various chert-beds, which, after Dr. G. J. Hinde (Phil. Trans. Roy. Soc. vol. clxxvi. 1885), are accepted as of sponge origin (deep-water deposits), and further by following the outcrop of the pebble-beds, described by Mr. C. J. A. Meÿer (Geol. Mag. for 1886, p. 15).

In Part 1 of the paper the author discusses the district to the west of the Mole, and endeavours to show that the view set forth in the Weald Memoir of the Geological Survey, to the effect that between Dorking and Leith Hill the lower horizons of the Lower Greensand undergo a change in composition, although possibly verbally correct, is geologically incorrect, since the lithological change is from south to north, from beds laid down in deep water to beds laid down in shallow water. In his communication to the Geologists' Association of last year, the author showed that the pebble-bed at the base of the Folkestone Sands was at Abinger intimately associated with the Bargate Beds; and he now states that he has identified this pebble-bed in the Dorking-Horsham road section, described by Prof. G. S. Boulger and himself in 1892, and at two other places to the east. The drifts of the same neighbourhood are then discussed, and it is found that at the top of and on both sides of the Lower Greensand escarpment, which, as stated by the Geological Survey, is here sandy throughout, there are gravels obviously deposited by a considerable stream consisting chiefly of Lower Greensand chert (entirely of Lower Greensand material) with, amongst the rougher material, lenticular beds of fine pebbles composed chiefly of débris from the Bargate Beds. Fragments of Lower Greensand chert have been obtained from the alluvium or from the beds of the streams now draining the Weald area to the south of Dorking. The soil over the Weald Clay as far south as Holmwood Common has everywhere yielded to the author fragments of the same chert. Hence it is argued that the chert-beds now seen upon Leith Hill to the west formerly existed over the Weald to the south of Dorking, and that the fragments now lying about the surface have been left by denudation, as described by Dr. G. J. Hinde (*op. cit.*). Since, however, as has been stated, the present Lower Greensand escarpment to the north consists of "sandy beds" only, there must be a lithological change from south to north (deep-water beds to shallow).

Part 2 of the paper is devoted to the district east of the Mole. Where the escarpment rises above the alluvium of the river the

author finds the Bargate Beds with pebbles (at Park Hill, Reigate) separated from the Folkestone Sands only by a thin bed of Fuller's Earth and a layer of sandy chert. The section is now first described; the dip has been observed and proved, and by measurement this pebble-bed is shown to lie at approximately the same horizon above the Atherfield Clay as when it was last seen west of the Mole. From Reigate eastwards to Tilburstow Hill the same beds are seen in the numerous hollow lanes and pit-sections. The pebble-beds are found approximately on a definite horizon; but whilst they become of less importance eastward, the overlying cherts, first seen at Reigate, become of greater importance in that direction. The thin bed of Fuller's Earth, also first seen at Reigate, thickens to the east likewise.

2. "On the Eastern Limits of the Yorkshire and Derbyshire or Midland Coalfield."[1] By W. S. Gresley, Esq., F.G.S.

The author attempts to throw light on the question of the easterly extension of the Yorkshire, Derbyshire, and Nottinghamshire coalfield beneath the newer rocks. He notices the general trend of the strata, the sizes of other British coalfields, the question of the origin of mountains, stratigraphical considerations, and the faults of the North of England. His object is rather to suggest what he believes to be novel ways of treating the subject than of reaching conclusions or locating limits.

3. "On some Phases of the Structure and Peculiarities of the Iron Ores of the Lake Superior Region." By W. S. Gresley, Esq., F.G.S.

The author has been studying heaps of ore brought from the region lying south-west of Lake Superior since 1890. He describes certain structural features of the ore-fragments, and discusses the evidences of mechanical movements and chemical alteration exhibited by these fragments.

CORRESPONDENCE.

THE GEOLOGICAL AGE OF THE ROCKS OF CHARNWOOD FOREST.

Sir,—As some uncertainty seems to hang about the origin of the name "Cambrian" as applied to the rocks of Charnwood Forest, will you allow me to make a short statement on the subject.

Having had occasion recently to refer to the Survey Memoir "On the Geology of the Leicestershire Coalfield," published in 1860, for another purpose, I turned to the passage dealing with the Charnwood Forest rocks; and as regards the origin of the name "Cambrian" as applied to them by the Survey, I thus wrote (p. 11): "In the absence of organic remains, these rocks have been referred by Professor Sedgwick and Mr. Jukes to the Cambrian period; amongst other reasons, from their resemblance to the rocks of this age in Wales. They have been described in detail by Mr. Jukes,[2]

[1] See also a paper on the same subject by Mr. Arnold Lupton, entitled "On the Geology of the West Yorkshire Coalfield" (Trans. Federated Inst. Mining Engineers, vol. vii. pt. 1, p. 137).

[2] Geology of Charnwood Forest, appended to Mr. Porter's History of Charnwood Forest.

and the Geological Survey of the range has been executed by my colleague Mr. H. H. Howell."

From this it will be seen that the age of these rocks was determined by no less an authority than Professor Sedgwick, the founder of the Cambrian system in Wales, supported by his friend and pupil Professor Jukes, and the ground of this determination is the resemblance of the Forest beds to the Cambrian beds of North Wales. Who amongst men should have been better able to recognize this resemblance than the distinguished founder of the Cambrian system? Nor was the opinion of Professor Jukes less entitled to weight, as Jukes spent several years of his Survey life amongst the old rocks of North Wales. For six months of my own Survey life I was his companion and pupil in the same region; and when a few years afterwards I had opportunities of examining the rocks of Charnwood Forest I was able to recognize the similarity of physical character, upon which Sedgwick and Jukes to a great degree relied in determining the geological age of the rocks; nor, notwithstanding what has been written by more recent authors, am I able to see good reason to alter the opinion I then held.

On reading over again the brief description of these rocks (as far as regards their non-plutonic masses), written at a time when their characters were vividly impressed on my mind, and when there was no dispute regarding their age, I see how similar is the description to one which might be applied to the Lower Cambrian beds of Merionethshire. "Crossing the axis we find the beds as far as Upper Black Brook, consisting of bluish-purple and green slates, of a coarse character, alternating with fine grits." The eruptive masses are then described, and their resemblance to the contemporaneous and intrusive masses of North Wales is pointed out (p. 12). Since these words were written much new light has been thrown on the composition and structure of the igneous masses by Professor Bonney and other petrologists. In 1860, the application of the microscope to thin sections of rock was scarcely begun; but the determination of the geological age of the whole series of the Charnwood rocks is not affected by microscopic definitions of the eruptive beds.

I have still much hope that fossils may yet be discovered in these old rocks. After the discovery of a Trilobite in the slates of Llanberis no one need despair; and any young geologist who desires to break fresh ground and win his spurs would do well to turn his attention to the slates of Charnwood Forest.

EDWARD HULL.

THE MAKING OF MOUNTAINS—A REPLY TO MR. MELLARD READE.

SIR,—May I ask you to allow me space to answer, as briefly as possible, a letter, in the GEOLOGICAL MAGAZINE for December, 1894, in which Mr. Reade attacks my theory on the making of mountains.

I must in the first place object to a statement implying that I have, to a certain extent, changed front. Mr. Reade remarks: "Mr. Vaughan *now* says, that he does not rely upon decrease of

volume due to pressure, but upon the transference of material," etc. I *never did* rely on decrease of volume, as Mr. Reade will see if he reads my original article more carefully, whereas, again and again, I insisted upon the main principle of transference of material.

Further on I read, that Mr. Reade has found out that the maximum elevation which could possibly be produced, according to my theory, is 1000 feet. Absurd though I believe this result to be, from my own calculations, I shall be most grateful if he will indicate his method of analysis, as the problem seems to present exceptional mathematical difficulties. Looking at the question in its simplest aspect the analysis employed must take account of each of the following phenomena:—

(1) The gradual contraction of a spherical cap with the consequent production of tension at its boundaries and frictional stress beneath.

(2) The inbending of the crust, and consequent outshear of material from beneath, with the necessary production of frictional stress. For this outsheared material must set up tensions and compressions in the superincumbent crust; the tensions being directed from the centre of the area outwards, and the compressions producing folding and ridging, principally at the boundaries, in the regions immediately preceding those of upheaval.

(3) The rate of contraction beneath the area, which will necessarily be made more rapid by the extension of material, and the consequent exposure of hotter layers to the cooling influence.

In fact, we have to deal with elevation due to distortion of the inner nucleus, and this cannot be limited to the amount by which the suboceanic shell can contract; for the viscous stresses due to outshear of subjacent material will compress the bounding parts of the area into folds and ridges. The only rough limit which can be assigned is, that the volume of the elevations above sea-level cannot exceed the volume of the ocean.

Another objection which my critic urges is that, since my theory demands that the surrounding continental crust should yield to the intrusion of rock underneath, the area could not bend inwards, because it has no anchorage. Here he has totally neglected the friction beneath the sinking area, arising from the outshear of material, which it seems to me will be powerful enough, not only to hold the area, but to fold and ridge its boundaries.

Lastly, Mr. Reade seems to argue that, granted the anchorage, the tension of rock would not be sufficient to allow of enough pressure to produce any tending inwards. In this reasoning he has entirely omitted the vastly important time factor; for, in reasoning on all geological phenomena, the length of time available is the most important point in the problem; since there are few geologists who will not grant that the making of a mountain range is spread over a vast number of years.

Now, it is well known that any practically solid body, acted upon by differential stresses, however small, will yield to their influence, if the time be sufficiently prolonged. Hence there is no need for the tension to exceed or even approach the breaking tension of steel.

To illustrate the great importance of the time factor, I will assume the extreme case, that entire rupture is immediately produced all round an isolated suboceanic area, in spite of the natural effect of the viscous friction beneath. The mechanical problem is then reduced to the following: An inner nucleus of highly heated material is surrounded by a spherical shell of rock. From this outer shell a spherical cap is cut out, reduced in area, and replaced, so that it does not quite fit and is in consequence entirely separated from the rest of the shell; every particle of shell and cap being subjected to the attraction of gravity tending towards the centre of the sphere.

The forces acting upon the non-fitting cap are simply its weight and the pressure of the subjacent nucleus; and it is to be noticed that, in the actual case, the weight of the suboceanic crust is almost certainly greater than that of the continental (see Pratt, "Figure of the Earth").

In the case of any portion of the rest of the shell the forces are: weight, resistance of nucleus, *together with* the supporting compression at its boundaries, arising from the fact that the area forms part of a continuous shell.

Hence the pressure on the nucleus will be greater under the suboceanic than under the continental crust. Thus, remembering that there is practically no limit to the time factor, transfer of material will take place slowly from beneath the oceanic to beneath the continental regions, and this the more easily on account of the high temperature at which the subjacent rocks exist.

I believe that I have now answered Mr. Reade's objections, which seem to me somewhat hastily conceived and dogmatically expressed; but I must confess to being unable to understand how the last sentence in his letter has any bearing on the points at issue. He says, alluding to my theory: "It seems to me *very like* the case of a man trying to lift himself up by pulling at the chair he is sitting upon." I cannot answer this objection, because I cannot imagine which is the man, nor which the chair beneath him; nor can I see how one is attempting to pull the other. As Mr. Reade has never made any reply to my criticism of the theory which he advocates, may I definitely ask him how he explains the undermentioned geological facts?

It is well known that, during many periods, great thickness of sediment have continuously accumulated, every part of which must have been laid down in shallow water; and that contemporaneously the area of denudation has been rising.

Does it not follow that, if two miles (say) of rock has been deposited continuously under shallow-water conditions, that the sea-flow must have been depressed through that distance, and that, consequently, an equivalent amount of subjacent rock has been squeezed out? I entirely fail to see how this is in accordance with Mr. Reade's theory, that, as sediment accumulates, the area of sedimentation rises; for, considering the slow rate at which deposition goes on, the elevational effects (if any) due to accession of heat must be contemporaneous with sedimentation.